Essays in Economic Development and Political Economy

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Abstract

The three chapters in this dissertation examine aspects of the relationships between transparency, government accountability, and the quality of public services. In the first chapter, I ask how ethnic diversity, or lack thereof, among polling station officials affects voting outcomes. I exploit a natural experiment occurring in the 2014 parliamentary elections in India, where the government mandated the random assignment of state employees to the teams that managed polling stations on election day. I find that the presence of officers of minority identities within teams led to significant shifts in vote share toward the political parties associated with these groups. Results suggest that the magnitude of these effects is large enough to be relevant to election outcomes. Using large-scale survey experiments, I provide evidence of own-group favoritism in polling personnel and identify the process of voter identity verification as an important channel through which voting outcomes are impacted.

The second chapter examines whether electronic procurement (e-procurement), which increases access to information and reduces personal interactions with potentially corrupt officials, improves procurement outcomes in India and Indonesia. We find no evidence of reduced prices but do find that e-procurement leads to quality improvements in both countries. Regions with e-procurement are also more likely to have winners come from outside the region. On net, the results suggest that e-procurement facilitates entry from higher quality contractors.

The third chapter studies the effects of the enactment across U.S. states of open meetings laws which ostensibly increase the public availability of information on legislator behavior.

As recent work shows that increased remoteness of capital cities in U.S. states is strongly associated with reduced accountability and worse government performance, I also investigate how the impacts of open meetings vary with state capital isolation. I find that open meetings increase spending on public goods and heighten confidence in state government on average. Heterogeneous impacts on incumbent vote share suggest that at both low and high levels of initial accountability, open meetings provide citizens with additional information that influences voting decisions.

Contents

List of Tables

List of Figures

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To Joseph, Yasmin, Xavira, and Khaled.

Introduction

In the first chapter of this dissertation, I ask how ethnic diversity, or lack thereof, in the teams of officers that manage polling stations on election day affects voting outcomes. I take advantage of a natural experiment in the Indian electoral setting in which government employees were randomly assigned to teams of polling station officials, generating exogenous variation in team composition in terms of religious and caste identity, both of which are strongly connected with political affiliation.

I use detailed polling station location information and unique officer assignment data for two districts in the state of Bihar covering more than 5.6 million registered voters in the 2014 national elections. I identify both direct effects within stations and spillover effects across stations of changes in team religious and caste composition on voting outcomes, demonstrating that the direct effects alone would be a biased estimate of the total impact.

To go further and shed light on underlying mechanisms, I embedded experiments in surveys of individuals randomly selected from the same populations of election officers and registered voters that were exposed to the election policy experiment. These data allow me to test for election officer bias and determine whether such bias leads to differential treatment of voters on election day.

I find strong evidence that own-group bias in election officers exists and influences the treatment of potential voters on election day. Using a vignette experiment in which polling station officers assess the likelihood that a hypothetical individual would be allowed to vote, I find that officers are significantly more likely to favorably assess the individual's qualification to vote when they are of the same religious/caste-group type. In addition,

the results of list experiments show that election officers treated potential voters differently based on religion/caste and explicitly attempted to influence citizens' voting behavior.

Consistent with the experimental results, I also find that minority potential voters at polling stations with no minority officers are on average significantly less satisfied with their overall polling station experience and less likely to be able to vote than are non-minority individuals. These effects disappear, however, at polling stations with minority officers present or when individuals possess a government-issued voter identity card, a method of identification which reduces officer discretion in determining voter eligibility. Furthermore, I confirm that the polling-station-level effects of team composition on voting outcomes are concentrated in areas with lower voter identity card coverage.

Counterfactual calculations indicate that the combined direct and indirect team composition effects are large enough to influence the outcomes of elections. Given that the two major political coalitions in this setting strongly differ in their propensities to field Muslim candidates, this also suggests that changes in officer team assignment could lead to significant shifts in the Muslim proportion of officeholders.

Fair and well-functioning elections are critical to maintaining the responsiveness of elected officials to citizens in democracies. While the related literature on election reforms has focused in large part on the benefits of advances in monitoring and voting technology, this paper is to my knowledge the first to provide rigorous evidence of the remaining importance of the identities of local-level election personnel. Indian elections are technologically advanced and their administration is highly regulated, indicating that bias in discretionary decision making of polling station officers can undermine the quality of service provision even at the present frontier of election practice.

The second chapter examines the impacts of the transition by developing country governments to electronic procurement (e-procurement) of goods and services. E-procurement can potentially address three common concerns with manual procurement practices: lack of access to bid information, collusion among bidders, and corruption. However, it is plausible that in low income settings, where information technology coverage and other aspects of

state capacity remain low, e-procurement can only effect limited change and can potentially make things worse.

We examine the impact of electronic procurement on public works projects in two large emerging economies: India and Indonesia. In India, we examine procurement practices for a federally funded rural road construction program which is implemented by state road departments. In Indonesia, we examine contract data from the national Ministry of Public Works for both construction and consulting (e.g., engineering management and design) contracts each year. The gradual roll-out of e-procurement (at the state-level in India and province-level in Indonesia) allows for a difference-in-differences strategy: We compare outcomes in states/provinces before and after the adoption of e-procurement, as well as in those continuing under manual procurement practices, allowing us to quantify the benefits or costs of the practice in both countries.

For both countries, we obtained administrative data on the complete universe of contracts from before and after e-procurement by scraping publicly available information from respective government websites. In addition, in India, we hand collected bidding data on tenders for four states which we use to supplement the administrative data.

We first show that, in both India and Indonesia, e-procurement increases the probability that the winning bidder comes from outside the region where the contract takes place. This is consistent with e-procurement decreasing the costs of submitting bids for those not physically present. We next examine the impact on the ultimate outcomes of interest: price, quality of construction, and timeliness.

We find no systematic evidence that electronic procurement lowers prices paid by the government. In contrast, e-procurement led to quality improvements, albeit along different dimensions in the two countries. A first measure of quality is time-overrun in project completion. In India, we observe no statistically significant changes in late works, while in Indonesia these declines are large and significant. A separate indicator of quality, only available for India, is an independent audit report on construction quality, which was conducted identically in roads completed under both e-procurement and traditional

procurement. According to this measure, we find that e-procurement leads to higher quality roads.

We then explore the degree to which the results are driven by improving outcomes among already winning bidders, as opposed to changing who wins. We find that after e-procurement, winning contractors in India tend to be those who have higher quality on average. In Indonesia, we find evidence that those contractors who win after e-procurement are systematically less likely to be late. This suggests that a key mechanism for e-procurement is allowing higher quality contractors to enter and win projects, rather than simply encouraging better performance from an existing set of contractors.

The fact that we observe changes on the quality margin, and that it occurs through changing which contractors win rather than the performance of a given set of winning contracts, suggests that the system prior to e-procurement was not necessarily selecting the most efficient firms, and that e-procurement may have improved efficiency even if it did not necessarily lower prices paid. It also suggests that the practice of giving contracts to the lowest price bidder likely contributed to greater inefficiencies on the quality margin. Overall, our findings provide qualified support to the view that e-governance can improve the provision of public services.

The third chapter provides empirical evidence on the effects of increased transparency of the actions of elected representatives in the U.S. state legislative setting. Increases in institutional transparency in democracies are commonly assumed to improve outcomes of policy making. While proponents of greater transparency emphasize the benefits of increased accountability and responsiveness of elected representatives, theory suggests that greater transparency need not lead to beneficial consequences for the general public. For example, it may be that, in contexts where it is difficult for the public to evaluate fully the consequences of representatives' choices, increasing observability of the actions themselves may increase the incentives of representatives to disregard valuable private information when choosing what to do. Given the theoretical uncertainty, an empirical investigation is valuable in helping to improve our understanding of which of the potential forces are

dominant in U.S. state legislatures.

I exploit variation in the timing of enactment of open meetings laws across U.S. states and employ a difference-in-differences estimation strategy. I examine the impact of open meetings on the total numbers of bills introduced and enacted and on the timeliness with which the state budget, arguably the most important piece of legislative output, is delivered. As a more direct measure of whether legislators are incentivized to take actions with important economic impacts, state government expenditure is considered as an outcome. I finally examine how open meetings influence citizens' perceptions of government and their behavior in elections to state legislatures.

In addition to considering the average impacts of open meetings on these outcomes, I investigate how the effects are mediated by the level of geographic isolation of the state legislature from its constituents, which recent work has shown to be robustly associated with weaker accountability and worse government performance. The effects of increasing transparency through open meetings may then differ in the isolation of the state government, due to differences across states both in the baseline level of quality of government and in the effective magnitude of the change in public informedness associated with open meetings.

I find that open meetings decrease legislative enactments, but do not impact the number of bills introduced or timeliness of budgets on average. Results suggest that open meetings significantly increase expenditure on public goods, and that these expenditure effects are concentrated in areas with greater state capital isolation, where government spending on public goods is lower to begin with.

Open meetings also shift citizens' perceptions of state government, such that in national-level Gallup surveys they are significantly more likely to express at least moderate confidence in the government on average. An examination of heterogeneity in impacts by capital isolation shows that the effects are driven by gains in low isolation areas. In states with more isolated capitals, open meetings actually increase the proportion of respondents choosing the lowest possible measure of confidence. Mirroring this pattern, I observe significant differences in the impact of open meetings on voting outcomes by the isolation of the capital.

Whereas incumbents in low isolation locations see their vote shares increase with open meetings, those where the capital is more remote experience a decrease in vote share.

The findings of this chapter suggest that open meetings in the U.S. state legislative setting are on average beneficial. Expenditure on public goods increases, concentrated in areas where spending of this type is the lowest to start. Additionally, citizens express greater confidence in the ability of state governments. The results also indicate that, even in environments with low initial levels of accountability, open meetings lead to shifts in voters' information and that they respond to its content. That is, weak transmission mechanisms to the public or low uptake by citizens do not appear to be binding constraints which prevent increased accessibility of information about legislator behavior from producing downstream effects.

Chapter 1

Enfranchising Your Own? Experimental Evidence on Polling Officer Identity and Electoral Outcomes in India

1.1 Introduction

Electoral malpractice and election day violence are common problems across the world, as the most recent round of the World Values Survey shows.[1] Figure 1.1 shows that at least twenty-five percent of respondents in more than one half of survey countries indicate that violence at the polling station is often a problem, and in nearly three quarters of countries that election officials are often unfair. The provision of well-functioning elections constitutes a critical public service. The ability of a country's citizens to cast votes in a free and fair setting is desirable in its own right, but is additionally important to the extent

[1]Round 6 was administered to representative samples of individuals across sixty countries between 2010 and 2014, but in only forty-two were election-related questions asked. This round was the first to include such questions.

that it increases the accountability of elected officials, with subsequent impacts on policy decisions and citizen welfare (Besley and Case 1993, Maskin and Tirole 2004).

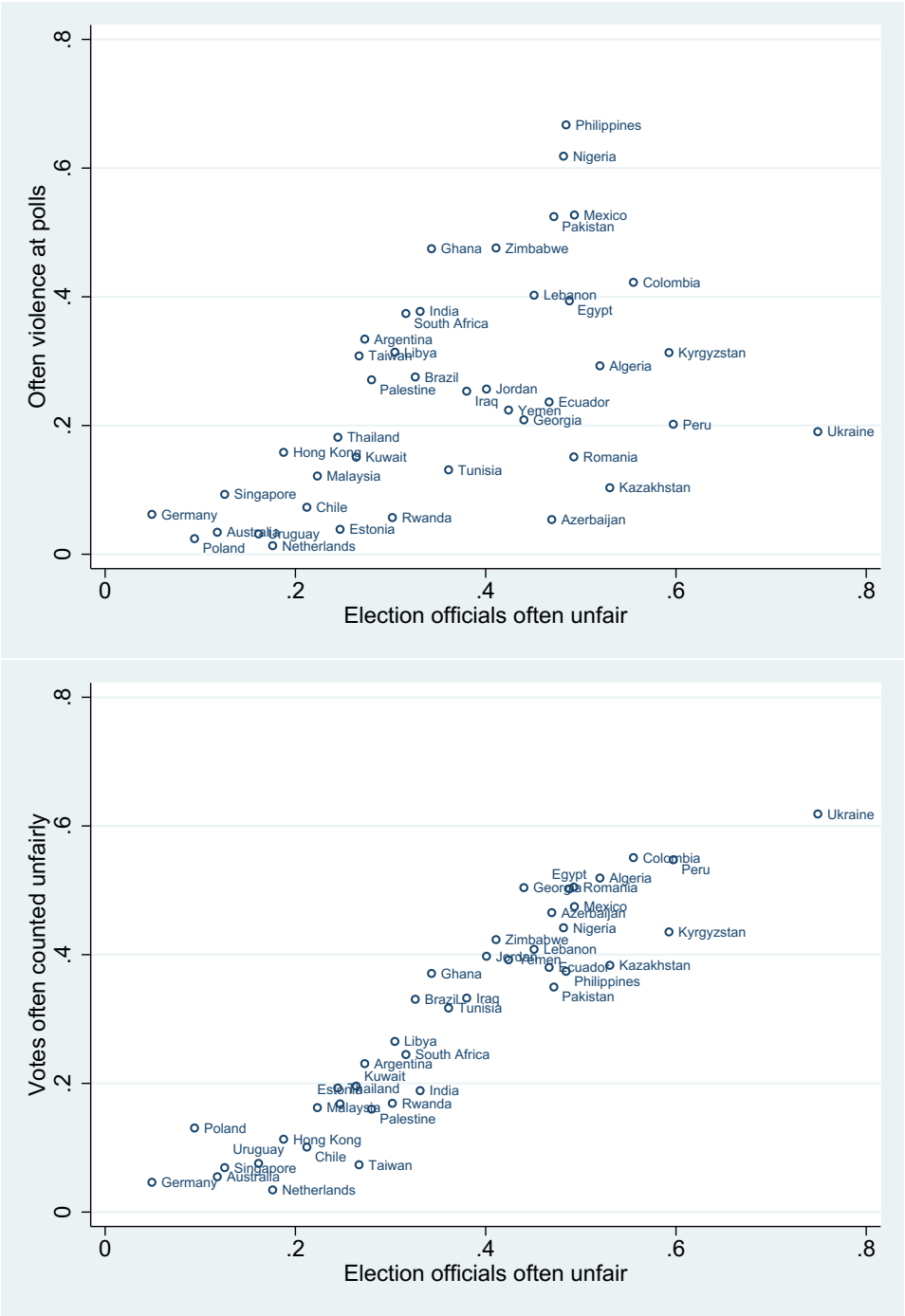
Election management and voting technology vary widely across countries, including a fundamental aspect of electoral administration, the staffing of polling stations on election day. Volunteers manage polling stations in the United States, while in Argentina randomly selected citizens work as polling station officials within their own municipalities. In India polling officials are randomly drawn from pools of government employees, and in Kenya polling officers are temporary, paid positions staffed through an open application process. Given the variety in election administration across countries and the frequent dissatisfaction of citizens with elections, there is clear need for causal evidence on what works in election reforms, particularly in relation to personnel management.

In this paper, I ask how ethnic diversity, or lack thereof, in the teams of officers that manage polling stations on election day affects voting outcomes. I take advantage of a natural experiment in the Indian electoral setting in which government employees were randomly assigned to teams of polling station officials. The government's assignment method generates random variation in team composition in terms of religious and caste identity, both of which are strongly connected with political affiliation. Largely in opposition to upper-caste Hindu influence, Muslims and Yadavs (a low-caste Hindu group) formed a political alliance in the state of Bihar in the mid-1990s, and this coalition remained operative in the most recent 2014 national elections.[2] The teams of officers I study contained at least one Muslim or Yadav approximately one third of the time, allowing me to identify the causal impacts of shifting from a "homogeneous" to "mixed" team of officers at a polling station.[3]

The random assignment circumvents otherwise confounding issues of selection in election officer placement at polling stations. A government may assign election personnel

[2]Wittsoe (2013) provides a detailed account of the state of the alliance over time.

[3]Due to the low proportions of Muslims and Yadavs among officers, teams that are fully Muslim/Yadav are not observed in my sample.



Notes: Measures computed using World Values Survey Wave 6 (2010-2014). "Election officials often unfair" is the weighted percentage of respondents in each country, when asked "In your view, how often do the following things occur in this country's elections?", answering "Not at all often" or "Not often" to "Election officials are fair", against the alternatives of "Very often", "Fairly often", or "Don't know/Not answer". "Often violence at polls" is the percentage answering "Very often" or "Fairly often" to "Voters are threatened with violence at the polls." "Votes often counted unfairly" is the percentage answering "Not at all often" or "Not often" to "Votes are counted fairly."

Figure 1.1: *Election administration difficulties by country*

with greater experience to manage more troubled locations in an effort to maintain neutrality. Alternatively, the ruling party may station supporters as officers in strategically important areas to influence outcomes in their favor. In either case, the assignment of officers would be endogenous to voting behavior. Conditional on the integrity of the randomization, which I test and confirm, the setting considered in this paper is not subject to issues of this type. An additional benefit of the study context is that the polling officer assignment policy had already been in place statewide for a decade at the time of the election under consideration, eliminating concerns that the estimated impacts reflect only partial equilibrium effects that may disappear once the policy is brought to full scale or as the government and political parties adjust to the change over time (Acemoglu 2010, Svensson and Yanagizawa-Drott 2012).

I study two districts in Bihar covering more than 5.6 million registered voters across 5,561 polling stations for the 2014 national elections. Using detailed polling station location information and unique officer assignment data, I identify both the direct effects within stations and the spillover effects across stations of changes in team religious and caste composition on voting outcomes. The omission of these cross-station effects could potentially bias the estimates of overall impact.

To go further and shed light on underlying mechanisms, I embedded experiments in surveys of individuals randomly selected from the same populations of election officers and potential voters that were exposed to the election policy experiment.[4] These data allow me to test for election officer bias and determine whether such bias leads to differential treatment of voters on election day.

This paper has three main results. First, I find that changes in team composition affect voting both within and across polling stations. The average vote share margin between the two major political coalitions is reduced on average by 2.3 percentage points, or 12.7 percent, when the officer team at a given polling station is mixed. This shift is driven by a significant

[4] "Potential voter" refers to registered voters who went to the polling station on election day with the intention of voting.

4.6 percent increase in votes for the minority-oriented coalition and a 4.1 percent decrease in votes for the non-minority coalition. In addition, I find that having a neighboring station that is mixed rather than homogeneous decreases the vote share margin by an average of 2.6 percentage points, demonstrating that the direct effects alone would be an underestimate of the total impact.

Second, I find strong evidence that own-group bias in election officers exists and influences the treatment of potential voters on election day. I measure bias using a vignette experiment in which polling station officers assess the likelihood that a hypothetical individual would be allowed to vote, based on a description where all information is held constant across respondents with the exception of the individual's name, which is varied randomly. Officers are 10 percentage points, or 25 percent, more likely to favorably assess the individual's qualification to vote when they are of the same religious/caste-group type. In addition, I find using separate list experiments that approximately 20 to 25 percent of officer and potential voter respondents agree that election officers treated potential voters differently based on religion/caste on election day, and roughly 5 to 10 percent that officers explicitly attempted to influence individuals' voting behavior.

Consistent with the experimental results, I find that Muslim/Yadav potential voters at polling stations with no minority officers are on average significantly less satisfied with their overall polling station experience and less likely to be able to vote than are non-Muslim/Yadav individuals. These effects disappear, however, at mixed team polling stations or when individuals possess a government-issued voter identity card, a method of identification which reduces officer discretion in determining voter eligibility. Furthermore, I confirm that the previously identified polling-station-level effects of team composition on voting outcomes are concentrated in areas with lower voter identity card coverage. Taken together my results suggest that religious/caste diversity within officer teams and the reduction of the scope for discretion in officer duties function as substitutes in improving the impartiality of election proceedings.

Third, I ask whether the combined direct and indirect team composition effects are large

enough to influence the outcomes of elections. Under reasonable assumptions, estimates from counterfactual calculations indicate that alternative officer assignment mechanisms would have changed the identity of the winning coalition in approximately 5 to 15 percent of races in the most recent national and state elections in Bihar. Given that the two major political coalitions strongly differ in their propensities to field Muslim candidates, these changes in election outcomes would have led to approximately a 25 percent increase in Muslim officeholders. Recent work finds that in India the election of Muslim legislators significantly improves child health and education outcomes (Bhalotra et al. 2014), further suggesting how officer team composition can have downstream effects on citizen well-being.

This paper complements the literature examining technology-centered approaches to strengthening elections. While technological innovations in the election setting have been shown to significantly impact electoral fraud (Callen et al. 2015), voter turnout (Marx et al. 2014), and even subsequent public service delivery and health (Fujiwara 2015), less progress has been made in understanding, holding the electoral setting otherwise constant, how the identities of election personnel may still matter.

My results additionally relate to work which finds that election observers reduce fraud at their posted polling stations when they represent non-politically affiliated international or domestic organizations (Hyde 2007, Ichino and Schündlen 2012), but may introduce additional bias when they themselves have partisan preferences (Casas et al. 2014). While this literature considers individuals external to the government who are explicitly tasked with monitoring polling stations, I focus on the government agents responsible for managing election proceedings themselves.

This paper also contributes to the body of research studying the negative impacts of ethnic fractionalization on government decision making and the provision of public goods (Easterly and Levine 1997, Alesina et al. 1999, Miguel 2004, Miguel and Gugerty 2005, Shayo and Zusman 2011). I provide micro-econometric evidence on an additional area, the administration of elections, in which heterogeneity in the ethnic composition of a population can lead to adverse effects on the quality of public service provision.

Finally, the implications of my work are also relevant to a literature examining potential discrimination against blacks and hispanics in the American electoral system. Recent research suggests that minorities in the US have different procedural experiences at polling stations on election day (Ansolabehere 2009, Atkeson et al. 2010, Cobb et al. 2012), have poorer perceptions of poll worker job performance (Hall et al. 2009), and receive lower quality information from local election administrators in response to requests about voting requirements (Faller et al. 2015). In addition, a 2014 U.S. government study states that "one of the signal weaknesses of the system of election administration in the United States is the absence of a dependable, well-trained corps of poll workers" (PCEA 2014). My results further underscore the relevance of dimensions of voter identity such as ethnicity to the quality of election-related service provision by local-level bureaucrats.

The paper proceeds as follows. The next section provides background on the historical and institutional context of the study, while Section 1.3 presents a conceptual framework. Section 1.4 describes the data and performs a randomization check. Section 1.5 presents the reduced-form impacts of team composition on voting outcomes. Section 1.6 provides empirical evidence on causal mechanisms. Section 1.7 considers alternative explanations and Section 1.8 concludes.

1.2 Background

1.2.1 Religion, caste, and politics

Over the last two decades, the dominant political parties in state-level politics in Bihar have been the RJD, BJP, and JDU. The RJD has traditionally enjoyed the support of an alliance between Muslims and Yadavs, a lower-caste Hindu group, which arose in large part in the mid-1990s in an attempt to counter upper-caste Hindu influence in the state (Wittsoe 2013). Muslims and Yadavs are sizeable constituencies in Bihar, making up approximately 17 percent and 14 percent of the population of registered voters, respectively (CSDS 2010). Between 2005 and 2013, the BJP and JDU parties were joined in a political alliance. The

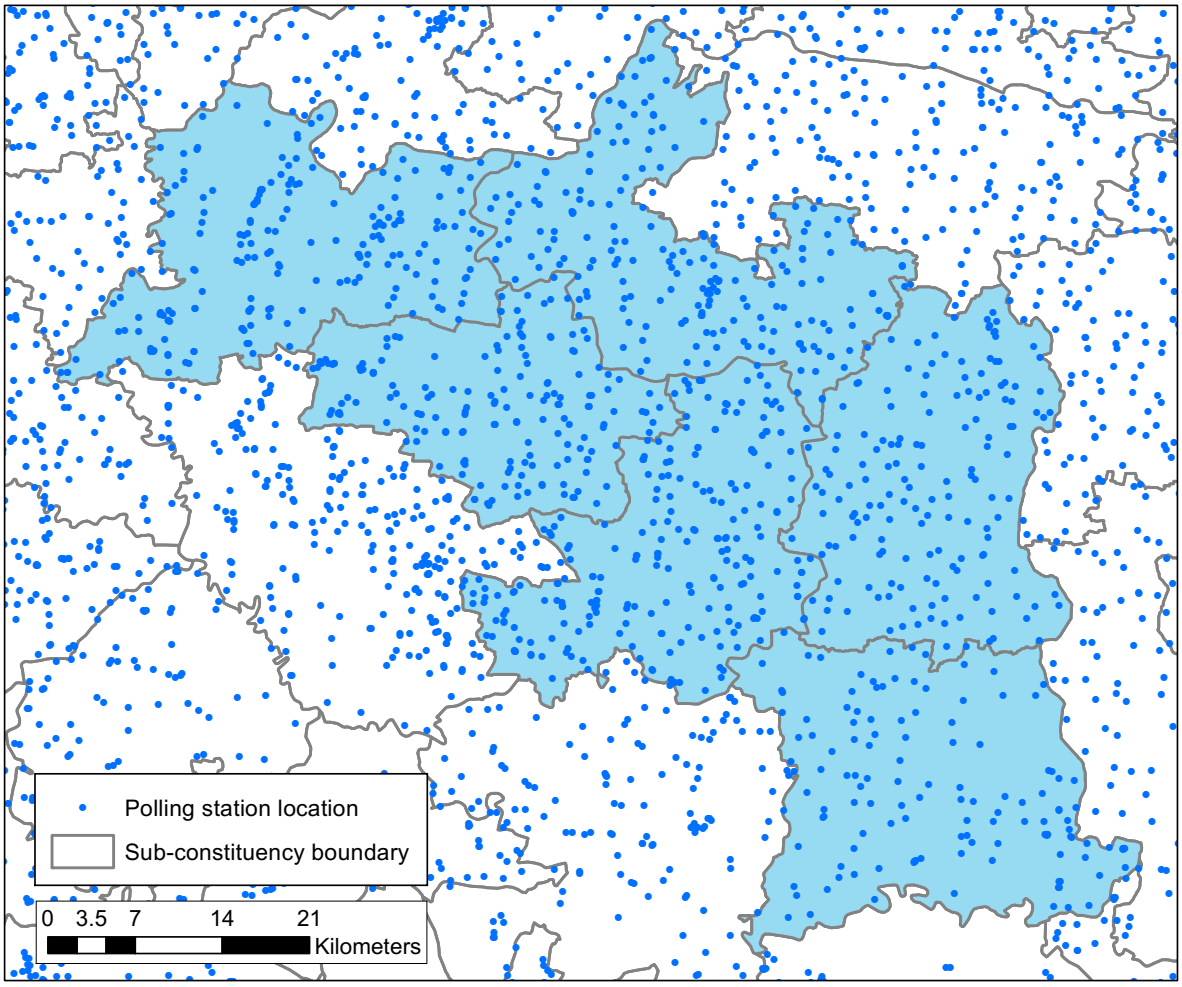
BJP was primarily supported by upper-caste Hindus, while the JDU relied more on the support of non-Yadav lower castes. The BJP-JDU alliance dissolved in the run up to the 2014 parliamentary election and, as a result, religion and caste were widely considered of high electoral relevance (Anuja 2013, Bhaskar 2013, Rukmini 2014).

The RJD and BJP subsequently each formed coalitions with other political parties and the JDU contested alone. Members within each coalition agreed prior to the elections not to field candidates in the same races. As upper-castes are less than 15 percent of the population in Bihar, the BJP increased its efforts to court low-caste Hindu voters. Post-polls for the 2014 elections indicate that only 19 percent of Muslims and 2 percent of Yadavs voted for the BJP coalition, while approximately 78 percent of upper-caste Hindus and more than 50 percent of other low-caste groups did so. Correspondingly, only 5 percent of upper castes and 10 percent of other low-caste groups, but 64 percent of both Muslims and Yadavs, voted for the RJD coalition (Kumar 2014a).

Given the strong connections between religious and caste identity and party affiliation, non-Muslim/Yadav officers are expected on average to be relatively politically inclined toward the BJP coalition over the RJD coalition, and vice versa for Muslim/Yadav officers. Section 1.3 discusses the channels through which shifting from a homogeneous to mixed polling officer team in terms of religious/caste composition may influence voting outcomes. I hereafter refer to the coalitions as simply the RJD and the BJP.

1.2.2 Administrative structure and randomized officer assignment

Bihar, with a population of roughly 100 million, is the third largest state in India and divided into 40 parliamentary constituencies (PCs), single member jurisdictions electing representatives to the national parliament via plurality rule. The PCs are further sub-divided into 243 assembly constituencies (sub-constituencies), each of which contains roughly 250 polling stations on average (see Figure 1.2). Registered voters receive a specific polling station assignment for each election and are only able to cast a vote at that station. Parallel to the electoral structure, the state's bureaucratic structure is divided into 38 districts. PCs



Notes: Shaded area in the figure indicates the extent of an example parliamentary constituency.

Figure 1.2: *Polling station distribution across example parliamentary constituency*

and districts often, but not always, fully overlap.[5]

A polling station is managed on election day by a presiding officer and typically three or four polling officers with distinct administrative responsibilities, detailed below.[6] Prior to elections, each district uses a proprietary government software program to randomly draw

[5]District administrators are responsible for managing election personnel assignment in those sub-constituencies falling within their districts.

[6]Four polling officers are assigned to polling stations with greater than 1200 registered voters in rural areas and 1400 registered voters in urban areas (21.1 percent of polling stations), and only two polling officers are assigned to polling station with fewer than 500 registered voters (0.7 percent of polling stations). In the case of four polling officers, the fourth polling officer shares the duties of the second polling officer. In the case of two polling officers, the presiding officer additionally assumes the duties of the third polling officer.

120 percent of the total number of required officers. Each polling team position has a distinct district-level pool of state government employees from which the officers are selected. After the completion of polling duty training, a subset of the individuals in each position pool are randomly assigned to a polling officer team in a designated sub-constituency. Officers are not assigned to sub-constituencies where they are registered to vote or are employed full time. The randomization is conducted in the presence of official observers assigned by the national office of the Election Commission of India (ECI), no more than seven days prior to election day.

A second randomization is conducted in which polling teams are assigned to specific polling stations. This assignment occurs the day prior to deployment of the teams to polling stations, timed so that they arrive the night before the election and no one has advance knowledge of who the officers at a given polling station will be. The software program also automatically generates team rosters with photographs in .pdf format.

1.2.3 Polling station procedures

Polling station officials are transported together in teams from the district headquarters to their polling stations, making officer absence relatively conspicuous and easy to track. This centralized transport, as well as the automated generation of officer rosters with photographs, also makes it more difficult for officers to report to a polling station different than that to which they were officially assigned or to have someone else impersonate them. If officers are absent from assigned duty without a documented excuse, they are subject to punishment by the ECI. Despite the attempts of the ECI to impose high costs on officers for non-compliance, it may still be that some proportion of officers do not report to their assigned polling stations on election day.[7] To the extent that this occurs, the estimates in this paper can be interpreted as intent-to-treat effects.

On election day, potential voters wait in line at their polling station and sequentially

[7]Official attendance data is not available, but the election officer survey results indicate that officers are absent from duty very infrequently.

interact with the first through third polling officers. The first polling officer verifies individuals' identities against the official list of registered voters, which has each individual's name, age, and, when available, a relative's name, voter identity card number, and photograph. Once a voter successfully confirms her identity with the first officer, her name is read out to the rest of the team. The second polling officer then stamps her finger with ink so that she may not vote more than once, obtains her signature or thumb impression in the official register, and gives her a paper slip with a serial number designating the order in which the voting compartment may be entered. The third officer then checks the voter's finger for ink, allows her into the voting compartment, and activates the electronic voting machine so that a single vote may be cast. Potential voters at the polling station do not necessarily interact with the presiding officer, who is tasked with the overall management and supervision of station activities.

1.2.4 Election fraud and policy responses

The problem of "booth capturing", as it is commonly known in India, in which a polling station comes under the control of a political party on election day, was a widespread occurrence as recently as the 2004 national elections (Rohde 2004).[8] The ECI implemented a number of policies in an effort to stem this type of election fraud. Elections may be staggered over multiple weeks across different regions within a state to maximize the available coverage of central police and paramilitary forces, observers, and camera recording equipment at sensitive locations. Additionally, electronic voting machines (EVMs), which were first used in Bihar during a 2004 nationwide rollout to all state and national assembly elections, were adopted under the general assumption that they are more secure than the

[8]Capturing may take place in a relatively peaceful manner, with local leaders standing near the voting machine to instruct voters on their choice of candidate and making their decisions public to a nearby crowd of supporters. Votes may also be cast for absent citizens and certain groups may be prevented from voting. Alternatively, more violent methods may be employed, with armed individuals hired by parties taking control of a polling station to cast false votes or steal the ballot box, or using explosives and gunfire to reduce turnout (Wittsoe 2013).

traditional paper ballot.[9] For instance, EVMs have a maximum rate allowed of five votes per minute, meant to increase the difficulty of casting large numbers of false votes, and are more difficult to transport and counterfeit than ballot boxes.

The multi-stage randomized assignment of polling station teams was employed state wide in Bihar beginning in 2004, and has since been adopted nation-wide, covering more than 814 million registered voters across 543 parliamentary constituencies. Among the assumed benefits of the adoption of randomization was a weakened ability of political parties to coordinate ahead of time with polling station officials or identify which locations would be the easiest targets for capture. These policies are generally viewed as having been successful in reducing the frequency of outright booth capturing. However, issues potentially remain with biased election officer behavior on election day or types of electoral fraud that occur in the longer term prior to elections, such as vote buying or intimidation. I focus in this paper on the former.

1.3 Team composition: channels of impact

1.3.1 Within-station effects

In a setting where officers may engage in biased behavior at the polling station, a change from homogeneous to mixed team composition could influence voting outcomes through a "checks and balances" channel. Polling station officials have two sets of duties on election day: administration of the identity verification and voting process; and maintenance of a neutral environment in the area immediately surrounding the station. In addition, the connection of religion and caste with political affiliation is well known in this setting and potential voter type is observable to election officers.[10] Given their own preferences, officers may then wish to treat potential voters at the polling station differently based on religion and caste, in an effort to influence either ability to vote or choice of candidate conditional

[9] For a criticism of this assumption in the Indian context, see Wolchok et al. 2010.

[10] Each potential voter's name is read aloud during the identity verification process.

on voting.

Relative to a benchmark homogeneous team of officials, whose preferences are more likely to be aligned, a mixed team may increase the probability of detection and punishment of team members that act with bias in their administrative duties, reducing the likelihood of such behavior. Officers within a team are stationed in close proximity, typically sitting adjacent to one another (see Appendix Figure A.1). Observability of actions across team members is therefore high and officers can lodge complaints to the ECI directly, with potentially severe career consequences for individuals found to have behaved improperly in the conduct of their duties. In addition to strengthening the deterrence effect stemming from the potential for future punishment (i.e. higher expected costs), the presence of an officer of different religion/caste on an otherwise homogeneous team may also lower the probability that attempts at influencing voting on election day are successful (i.e. lower expected gains), further weakening the incentives of officers to engage in biased behavior.

The verification of voter identity prior to the casting of votes necessarily involves discretionary decision making by election officials. The judgement calls involved in this process may give officers the ability to successfully influence voting outomes with a lower probability of punishment as compared to actions that can be identified as improper with greater certainty.[11] As such, this step may be particularly susceptible to biased officer behavior, resulting in the disenfranchisement of qualified potential voters or enfranchisement of unqualified individuals.

The scope for officer discretion in the identity verification process, however, is heavily influenced by the identification documents that potential voters possess. The government-issued voter identity card is the officially preferred and least controvertible form of identification (Appendix Figure A.2 provides an example of the card). While eleven other sets of documents are allowed on election day, their use may provide greater discretionary cover to biased officer behavior during voter identity assessment. Potential voters may be less

[11]Guidelines from the ECI on election day management of polling stations even state that "minor errors in the EPIC [voter identity card] and electoral roll may be ignored and overlooked."

certain about what constitutes a valid alternative means of verifying identity, making them less likely to dispute officer judgement regarding their qualification to vote or increasing their susceptibility to influence in choice of candidate (e.g. if they are reciprocal individuals and feel as if they are receiving a favor in being allowed to vote). The potential monitoring benefit provided by a shift from homogeneous to mixed officer team composition may then be particularly important in situations where voter identity cards are less common.

The officer team is also responsible for maintaining a neutral environment in the area immediately surrounding the polling station. More specifically, any activities which may influence potential voters, such as canvassing of votes or disorderly behavior, are officially prohibited within one hundred meters of the polling station. If all officers on a team are of the same type, they may selectively allow agents of the political coalition with which they are aligned to engage in such behavior within that range of the station. As mixed team composition may weaken the incentives of officers to behave with bias, the likelihood that agents from both coalitions are prevented from violating neutrality could increase. In sum, if a homogeneous officer team behaves with bias and relatively favors one coalition, shifting to a mixed team would be expected to decrease votes for the previously favored coalition (here the BJP) and/or increase votes for the other coalition (here the RJD), with ambiguous predictions on total turnout.

Second, in the absence of biased behavior on the part of officers, introducing heterogeneity into polling station teams may influence voting through a "team performance" channel. The literature on teams and heterogeneity has highlighted the potential tradeoff of benefits associated with a greater diversity of skills and information against increased communication and coordination costs and reduced motivation (Prat 2002, Hamilton et al. 2003, Marx et al. 2015). Changes in the overall productivity of the officer team may affect the length of waiting time and consequently the proportion of potential voters willing to incur this cost of voting. In this case, impacts would be expected on total turnout, with effects on the votes received by each coalition in the same direction.

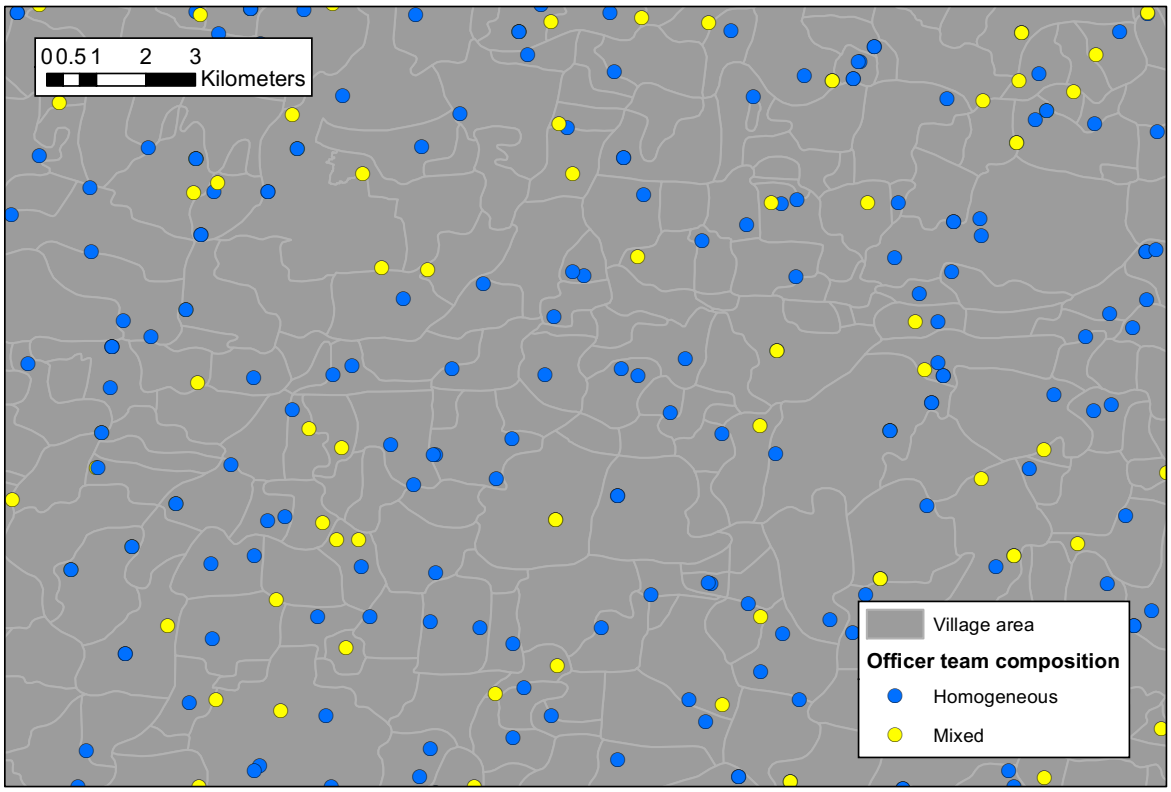
Finally, the identities of the election officials with whom potential voters interact at the

polling station may impact voting behavior through an "identity salience" channel. The behavior of voters has been shown to be sensitive to small changes (Gerber and Rogers 2009, Shue and Luttmer 2009, Bryan et al. 2011), and, even if officer actions are unaffected by team composition, the religion and caste of the election officials present on election day may be discerned by potential voters and influence their behavior. Effects of this type would be expected primarily to influence the choice of candidate, rather than the extensive voting margin.

1.3.2 Cross-station spillovers

In addition to impacting voting within a given polling station, the composition of an officer team may affect other stations, especially in settings where stations can be located within a short distance of one another (see Figure 1.3 and Appendix Figure A.3). Accounting for the possibility of these cross-station effects is important when calculating the total impact of changes in team composition, as their exclusion could bias the overall estimates downward or upward.

If a polling station is more strictly managed in terms of maintaining a neutral environment under mixed officer composition, the ability of local political agents to influence proceedings there could be reduced. These individuals may then intensify their focus on other stations which are more amenable to their activity, leading to "displacement effects" (Ichino and Schündlen 2012) that reduce the magnitude of the total impact on voting outcomes. The effects of more impartial management could alternatively spill over positively to nearby stations. Informational spillovers about what constitutes sufficient documentation for identity verification may take place across potential voters in neighboring polling stations, or the presence of officers of different types on teams in close proximity may serve a monitoring role as within teams. In these cases, mixed team composition could yield additional "chilling effects" (Callen and Long 2015) that increase the magnitude of the total effect. It is also possible that both displacement and chilling effects occur, but over different distances from a given polling station. Chilling effects would be expected to occur



Notes: Each circle represents a polling station, with the color signifying whether the officer team was homogeneous or mixed in composition. Figure presents an example area.

Figure 1.3: *Variation in officer team composition across polling stations*

across polling stations in closer proximity, while displacement effects could take place over longer distances.

1.4 Data

1.4.1 Administrative data

Administrative data on polling officers was acquired for two districts in Bihar for the 2014 elections, covering 23,384 officials posted across 5,561 polling stations. The data include officer name, team and position assignment, and, for a subset of officials, age and monthly salary. This information allows for inference of the religious and caste composition of each polling station team, described in greater detail in Section 4.3. Polling stations with at least one Muslim or Yadav officer are defined as "mixed", as opposed to "homogeneous", team polling stations.

Polling station level electoral returns were obtained from the website of the Office of the Chief Electoral Officer (CEO), Bihar. The main outcomes of interest generated from this data are the log numbers of votes received by each of the two main coalitions and cast in total, and the vote share margin between the coalitions. Sub-constituency-level measures of voter identity card possession were also acquired from the CEO website. Due to political sensitivity, religious composition statistics are not released by the government below the sub-district level. In order to generate new measures of electorate religious and caste composition at the polling station level, publicly available online lists were scraped covering the approximately 5.6 million registered voters in the two districts for which officer assignment data was available.

For the analysis of cross-station spillover effects, I use the polling station GPS coordinates from the dataset of Susewind (2014). As polling station identifier numbers change across elections and those in the dataset reflect the 2010 election cycle, stations were then hand matched by name, achieving a 94.5 percent match rate. The non-matches come almost entirely from new polling stations created due to increases in the number of registered

voters. I also use 2011 census village shapefiles acquired from MLInfoMap to match polling stations to villages.

1.4.2 Survey data

Between May and September 2015, surveys of potential voters and election officers from the 2014 elections gathered information on socio-demographic characteristics and election-related experiences. Experimental modules, discussed in more detail in Section 1.6, were additionally included to generate measures of officer bias. The surveys were conducted in one of the two districts for which officer assignment data was available.

For the survey of potential voters, a total of 4,320 individuals across 360 polling stations were sampled. In each of the 5 sub-constituencies in the district, 36 mixed and 36 homogeneous team polling stations were randomly selected, stratifying by whether the Muslim-Yadav proportion of the population was above or below the district-level median. For each of these polling stations, three Muslim and two Yadav registered voters were randomly chosen from the list of registered voters, if possible, along with seven randomly selected registered voters inferred as neither Muslim nor Yadav.

A total of 915 officers across 610 polling stations were sampled for the survey of election officers. 61 mixed and 61 homogeneous team polling stations in each of the 5 ACs were chosen randomly. One Muslim or Yadav officer and one non-Muslim, non-Yadav officer were then randomly selected from each mixed team, while a single non-Muslim, non-Yadav officer was randomly chosen from each homogeneous team.

Willingness to participate was high for both surveys: greater than 98 percent of contacted individuals in each agreed to be surveyed. Consent is not significantly correlated with voter or officer religion/caste, nor the overall composition of the team at the polling station to which they were assigned. Appendix Section A.1 provides additional details on the survey sampling methodology.

1.4.3 Inference of religious and caste identity

The categorization of election officers and registered voters as Muslim, Yadav, or neither is inferred from name. The Anthropological Survey of India's *People of India* (POI) series lists common surnames as well as religion and caste for 261 distinct communities identified as inhabiting Bihar. As surnames may be associated with multiple communities, potentially of different religious or caste affiliations, individuals are categorized as Muslim if their surnames match one listed in the POI that is associated only with Muslim communities. Individuals are also identified as Muslim if their name had components of clear Islamic origin, e.g., "Raiyaz" or "Mohammed". I categorize as Yadav those individuals with the surname "Yadav", as the majority of the members of the caste are so named and the surname is not associated with other communities. The lists of registered voters also provide the name of a relative for each individual (typically a father in the case of males or unmarried females, and husband in the case of married females). Given strong norms of marrying within religion and caste group in the region, I also categorize registered voters as Muslim or Yadav if their listed relative was inferred as falling into one of these categories. To the extent that individuals are misclassified, estimates of the impact of Muslim/Yadav officer presence will be biased toward zero.

1.4.4 Identification and randomization check

In the two sample districts, between 8.3 and 9.3 percent of officers in each team position are Muslim/Yadav, yielding 32.3 percent of polling stations with at least one Muslim/Yadav officer (i.e. mixed team). As officers within a district are not assigned to sub-constituencies in which they are registered to vote or work full time, a sub-constituency with a larger population proportion of Muslim/Yadav officers relative to other constituencies within the same district, for example, could then receive a lower proportion of Muslim/Yadav officers assigned to its polling stations, potentially mechanically leading to correlations between team composition and voting outcomes. However, it is still the case that each polling station *within* a sub-constituency is equally likely to have Muslim/Yadav officials posted to the

officer team. I therefore exploit only within-sub-constituency variation in team composition by including sub-constituency-level fixed effects in my subsequent analysis. In addition, because the likelihood of Muslim/Yadav presence on a team is increasing in the number of officers, which is itself determined by the number of registered voters assigned to the polling station, I include fixed effects for team size.

A remaining concern is the validity of the government's implementation of the random assignment. As a randomization check, I examine whether polling stations with mixed composition teams differ significantly in pre-election dimensions potentially correlated with voting outcomes, using the specification:

$$Y_{pc} = \mu_c + \theta_o + \beta Mixed_{pc} + \epsilon_{pc,} \tag{1.1}$$

where p is a polling station in sub-constituency c, μ_c are sub-constituency-level fixed effects, and θ_o are fixed effects for the number of polling team members. Y_{pc} is an outcome of interest, and $Mixed_{pc}$ is an indicator variable taking value 1 if at least one polling team member is Muslim/Yadav and 0 otherwise.

In Panel A of Table 1.1, I consider whether the size or composition of the electorate differs across homogeneous and mixed team polling stations. The average polling station has roughly 1,000 registered voters of which 46 percent are female and 13 percent are Muslim or Yadav, with no significant differences by team composition. In Panel B, I examine station-level electoral results from the previous 2010 elections to the state assembly. As the number of polling stations increases over time due to growing numbers of registered voters, it is not possible to fully match polling stations across elections. For each 2010 election-related variable, I therefore take the average value across all polling stations within the same immediate location in 2010 and assign it to each polling station in that location in 2014.[12] Additionally, a small proportion of polling stations were established in new locations for the 2014 election and so cannot be matched to previous elections.[13] I observe

[12]Section 1.5.2 provides greater detail on the identification of locations.

[13]The total number of polling stations across Bihar increased by 5.9 percent between the 2010 and 2014

no significant differences in the log votes previously received by either coalition or in total, or in the vote share margin between the coalitions.[14]

I next test for balance in the samples of surveyed election officers and potential voters in Panels C and D. Election officers are on average 43 years old, and the majority are college educated (70%) and have prior polling station experience (66%).[15] None of the officer characteristics differ significantly with team composition.[16] The sample of potential voters is approximately 44 percent Muslim/Yadav (by construction), 40 percent literate, and 55 percent female. While respondents from homogeneous team polling stations are more likely to be female (58 versus 54 percent), the other characterstics considered do not differ significantly by team type, and I control directly for gender when applicable in the analysis that follows.

Appendix Table A.1 tests for differences by team composition in the spatial distribution of surrounding polling stations. Polling stations have an average of 1.2 immediate neighbors (ranging between 0 and 8), 0.39 being mixed team (ranging between 0 and 4). Neither of these characteristics differ significantly across team types, nor do the average numbers of total or mixed team polling stations within 0.25 kilometers, between 0.25 and 0.75 kilometers, or within the same or neighboring villages. Finally, Appendix Table A.2 shows that the assignment of a Muslim/Yadav officer to a given position is not significantly correlated with officer type in the other positions within that team.

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elections.

[14]Observation numbers change across the previous election outcomes because the coalitions (as defined in 2014) fielded candidates in different numbers of constituencies in 2010.

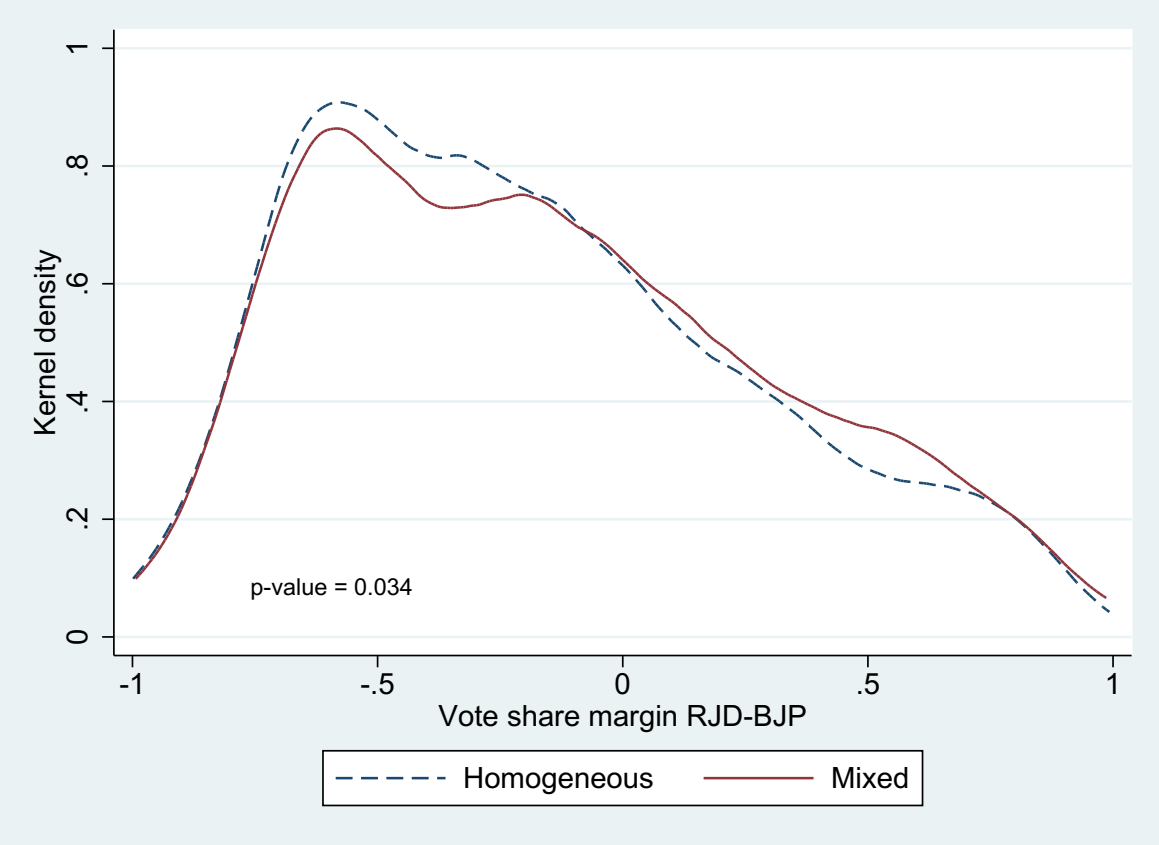
[15]Election officers are officially required to be male, with the rare exception of certain heavily Muslim areas where female officers may be used to interact with the female population. The sample area contains no stations of this type.

[16]By definition, homogeneous officer teams do not contain Muslim/Yadav officers. Therefore balance tests across team types of officer characteristics are necessarily restricted to the sample of non-Muslim/Yadav officers. Potential differences in characteristics across officer types are considered in Section 1.7.

Table 1.1: *Randomization check*

	Homog. team (1)	Mixed team (2)	Difference (3)	p-value (4)	Obs. (5)
Panel A. Electorate characteristics					
Ln total registered voters	6.873	6.905	0.009	0.160	5,561
	[0.314]	[0.305]	(0.007)		
Share female registered voters	0.463	0.463	0.000	0.864	5,561
	[0.023]	[0.022]	(0.001)		
Share Muslim/Yadav registered voters	0.128	0.135	0.005	0.312	5,561
	[0.172]	[0.175]	(0.005)		
Panel B. Prior election (2010) characteristics					
Ln total votes	6.061	6.057	-0.007	0.412	5,275
	[0.332]	[0.319]	(0.009)		
Vote share margin RJD-BJP coalition	-0.287	-0.272	0.000	0.992	3,947
	[0.378]	[0.376]	(0.009)		
Ln votes RJD coalition	3.941	3.945	-0.009	0.694	5,246
	[1.424]	[1.403]	(0.023)		
Ln votes BJP coalition	4.940	4.901	-0.003	0.899	3,946
	[0.995]	[1.019]	(0.025)		
Panel C. Officer characteristics					
Age	42.313	43.264	0.910	0.294	517
	[9.781]	[9.677]	(0.866)		
College graduate	0.695	0.675	-0.014	0.728	516
	[0.462]	[0.469]	(0.041)		
Ln monthly salary	9.539	9.584	0.047	0.371	503
	[0.609]	[0.562]	(0.053)		
First time officer	0.342	0.325	-0.017	0.686	511
	[0.475]	[0.469]	(0.042)		
Panel D. Registered voter characteristics					
Muslim/Yadav	0.430	0.446	0.016	0.306	3,903
	[0.495]	[0.497]	(0.016)		
Age	45.402	45.453	0.073	0.900	3,877
	[16.844]	[16.429]	(0.577)		
Female	0.580	0.537	-0.043	0.008	3,903
	[0.494]	[0.499]	(0.016)		
Literate	0.385	0.413	0.028	0.107	3,901
	[0.487]	[0.493]	(0.018)		
Household head	0.458	0.463	0.006	0.730	3,903
	[0.498]	[0.499]	(0.018)		
Ln monthly household income	8.212	8.254	0.038	0.286	3,326
	[0.828]	[0.827]	(0.035)		
Voter identity card possession	0.945	0.940	-0.004	0.647	3,903
	[0.228]	[0.237]	(0.009)		

Notes: Columns (1) and (2) report variable means with standard deviations in brackets for homogeneous and mixed officer teams. Column (3) reports the coefficient from an OLS regression where the listed outcome is regressed on an indicator for polling station mixed team composition and column (4) reports the associated p-value. Panels A and B also include sub-constituency and number of officer fixed effects. Prior election characteristic outcome values are based on the average value across all polling stations from 2010 in the same location as the 2014 polling station, as the total numbers and locations of polling stations change across election cycles. 2014 coalition definitions are used. Panel C is restricted to non-Muslim/Yadav officer respondents, due to the definition of mixed teams. Additionally included are sub-constituency fixed effects. Panel D considers registered voter respondents and additionally includes strata fixed effects (sub-constituency and above-below district-level Muslim/Yadav registered voter percentage median). *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.



Notes: The figure plots kernel density estimates of the polling station-level vote share margin between the RJD and BJP coalitions, separately for polling stations with homogeneous (dashed line) and mixed (solid line) teams of polling stations officers. Estimated using an Epanechnikov kernel. The p-value is computed using the Kolmogorov-Smirnov equality-of-distributions test for the two groups of polling stations.

Figure 1.4: *Empirical distribution of coalition vote share margins by team composition*

1.5 Reduced-form impacts on voting outcomes

1.5.1 Within-station effects

Does the presence of Muslim/Yadav officers on polling station teams change voting outcomes? Figure 1.4 plots the distribution of the polling-station-level vote share margin between the RJD and BJP, separately by team type. The average vote share of the RJD relative to that of the BJP is lower for homogeneous teams, where the equality of the distributions can be rejected at the 5 percent level.

I further examine impacts on voting by estimating equation (1.1), including polling-station-level controls for the log number of registered voters and the share categorized

Table 1.2: *Impacts of randomized officer team composition on voting outcomes*

	Ln votes RJD (1)	Ln votes BJP (2)	Vote share margin RJD-BJP (3)	Ln total votes (4)
Panel A. Within-station effects				
Mixed team	0.046*	-0.041*	0.023**	0.001
	(0.027)	(0.021)	(0.010)	(0.008)
Muslim/Yadav registered voter %	0.031***	-0.030***	0.015***	-0.000**
	(0.001)	(0.001)	(0.003)	(0.000)
Ln total registered voters	1.008***	1.177***	-0.060***	0.935***
	(0.060)	(0.048)	(0.023)	(0.018)
Observations	5,535	5,549	5,552	5,552
Homogeneous team mean [SD]	4.451	5.143	-0.181	6.180
	[1.198]	[0.969]	[0.452]	[0.402]
Panel B. Cross-station spillovers				
Mixed team	0.045*	-0.040*	0.023**	0.000
	(0.027)	(0.021)	(0.010)	(0.008)
Number mixed team neighbor stations	0.031	-0.042**	0.026***	0.003
	(0.025)	(0.019)	(0.010)	(0.008)
Total neighbor stations	-0.044***	0.046***	-0.032***	-0.017***
	(0.014)	(0.010)	(0.005)	(0.005)
Observations	5,535	5,549	5,552	5,552
Number locations	3,619	3,619	3,619	3,619

Notes: All columns report OLS estimates from regressions at the polling station level of the listed variable on an indicator for mixed team composition. Additionally included are sub-constituency and number of officer fixed effects and controls for Muslim/Yadav share of registered voters and log total registered voters. Standard errors clustered at the station level. In Panel B, variables for the numbers of total and mixed composition team neighboring polling stations are also included. Neighbor stations are polling stations within the same location (building/compound) as a given polling station. Standard errors in Panel B clustered at the location level. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

as Muslim/Yadav to improve statistical precision.[17] In Table 1.2, column (1) of Panel A shows that changing from a homogeneous to mixed officer team significantly increases the votes received by the RJD by 4.6 percent on average. I also observe a significant 4.1 percent decrease in BJP votes in column (2). In column (3), I consider the combined impact on the vote share margin between the RJD and BJP and find that mixed team composition significantly narrows the gap between the coalitions by 2.3 percentage points, or 12.7 percent.

Consistent with the strong connections of religion and caste to political affiliation in

[17]Results are robust to the exclusion of these covariates.

this setting, I also observe that a 1 percentage point increase in the Muslim/Yadav share of registered voters at a polling station is associated with a 3 percent increase in RJD votes and 3 percent decrease in BJP votes. Changing from a homogeneous to mixed team of officers therefore has roughly the same impact as increasing the Muslim/Yadav share of registered voters by 1.5 percentage points, where the overall average across sample polling stations is 13 percent. Finally, while column (4) indicates that mixed team composition has no average effect on the log total votes cast, I am unable to rule out effects of approximately 1.6 percentage points in magnitude in either direction,[18] and, as described earlier, the expected impact of changing composition on total votes is ambiguous.

1.5.2 Cross-station spillovers

I next test for spillover effects of team composition across polling stations in close proximity. Stations are defined as neighbors if their locations match in the administrative data,[19] or if they are within 0.1km based on the available GPS coordinates. I exploit the fact that, for each polling station, the officer assignment mechanism also generates random variation in the proportion of neighboring stations with mixed officer teams. Similar to Miguel and Kremer (2004) and Callen and Long (2015), I estimate spillovers with the specification:

$$Y_{pc} = \mu_c + \theta_o + \beta Mixed_{pc} + \gamma T_{pc} + \phi N_{pc} + \mathbf{X'_{pc}}\lambda + \epsilon_{pc}, \tag{1.2}$$

where N_{pc} is the number of neighbors of polling station p in constituency c, and T_{pc} is the number of these neighbors with a mixed officer team. Standard errors are clustered at the location level. Impacts associated with polling station density are captured by N_{pc}. Conditional on this density, the number of neighbors with mixed composition teams is

[18] Appendix Table A.3 considers whether impacts vary significantly by: the position within a team in which Muslim/Yadav officer presence occurs, or the presence of single versus multiple Muslim/Yadav officers. Significant differences are not found across positions or by number. Appendix Table A.4 additionally shows the absence of significant heterogeneity in impacts by share Muslim/Yadav registered voters.

[19] For example, a group of polling stations may be listed in the administrative data as situated in "K L Primary School (South Part)", "K L Primary School (North Part)", and "K L Primary School (Middle Part)" and would be categorized as neighbors.

randomly determined.

The within-station direct effects of mixed team composition on voting outcomes are given by β, while γ is the average cross-station spillover effect of a mixed team neighbor. Note also that, since the team type at each polling station with a given number of neighbors is orthogonal to the number of those neighbors that are mixed team, the estimates of the within-station impacts of changes in team composition should be unchanged from equation (1.1).

To extend the consideration of spillovers to longer distances, I use two different approaches. First, I supplement equation (1.2) with the variables $N_{pc}^{0.25km}$ and $N_{pc}^{0.25-0.75km}$, the number of non-neighbor polling stations within 0.25km and between 0.25-0.75km of polling station p, and $T_{pc}^{0.25km}$and $T_{pc}^{0.25-0.75km}$, the numbers of such polling stations with mixed composition teams.[20] Second, while this specification allows the impact of team composition on other stations to vary with linear distance, it may also be that a more meaningful distinction is captured by administrative boundaries. I therefore employ a specification which augments equation (1.2) with variables for the total and mixed team numbers of non-neighbor polling stations within the same village as polling station p, N_{pc}^{vill} and T_{pc}^{vill}, and neighboring villages, N_{pc}^{nei} and T_{pc}^{nei}.[21]

The estimates of equation (1.2) in Panel B of Table 1.2 identify the occurrence of chilling effects across polling stations in close proximity. Columns (1) and (2) show an imprecisely estimated 3.1 percent increase in RJD votes and a significant 4.2 percent decrease in BJP votes associated with a change in a neighboring polling station from homogeneous to mixed team composition. In column (3), the combination of these two effects yields a highly significant 2.6 percentage point cross-polling-station shift in vote share toward the RJD away from the BJP. As expected given the randomization, the point estimates on the within-polling station

[20]The sample for this specification is slightly reduced, as it excludes polling stations which could not be matched to the 2010 polling station GPS coordinates.

[21]As the top 1 percent of the distribution of villages in terms of polling stations has a mean of 98.8 as compared to the overall mean of 2.4, I trim the sample for this specification to exclude polling stations located in or neighboring these villages, which are also urban and large in area relative to typical villages.

mixed team indicator are unchanged as compared to those from equation (1.1).

The results of tests for spillover effects over greater distances, defined in linear distance and village boundaries, are shown in Panels A and B of Appendix Table A.5. While both the within-station and cross-neighbor effects of team composition remain significant, the estimates show no evidence of chilling or displacement effects over longer ranges.

1.6 Channel of impact: officer bias in discretionary decisions

Immediately prior to casting a vote at the polling station, each citizen must confirm her identity as matching an individual on the list of registered voters at that station. The verification process necessarily involves discretionary decisions by election officers, and so is potentially susceptible to the influence of officer bias. In this section, I provide evidence indicating that the process of voter identity verification is a major channel through which team composition and officer bias interact to impact voting outcomes.

1.6.1 Vignette experiment: own-group bias in election officers

First, I test for election officer own-type bias in the evaluation of voting eligibility, using a vignette experiment embedded within the survey of officers. I examine whether, holding all other information constant, potential voters are more likely to be assessed by an election officer as qualified to vote if they are of the same type as that official. Vignette experiments have been used previously to address research questions in the electoral setting (Carlson 2010, Banerjee et al. 2014) and are methodologically similar to the randomized CV experiment approach that has been employed in the labor market discrimination literature (Bertrand and Mullainathan 2003, Banerjee et al. 2009).

Each respondent was read a vignette describing a hypothetical individual attempting to vote, with the wording identical across respondents with the exception of the individual's name, which was randomly assigned.[22] Respondents were then asked to indicate the

[22]The vignette question was worded as: "Please consider the following situation: A voter named [RANDOMLY ASSIGNED] arrives at the polling station without an EPIC card but has a government voter's slip

likelihood on a 4-point scale that the individual in the vignette would be able to cast a vote. Each officer respondent was randomly assigned one of nine possible voter names. Three names each were chosen to signal Muslim, Yadav, or Brahmin (the highest of Hindu castes) identity in the hypothetical voter.[23] To examine whether an officer's evaluation of the likelihood of a potential voter's ability to cast a vote is influenced by whether that individual is of the same type as the officer, I use regression specifications of the form:

$$Y_{qpc} = \mu_c + \varphi_n + \pi_v + \theta Match_{qpc} + \mathbf{X'_{qpc}}\lambda + \epsilon_{qpc}, \tag{1.3}$$

where Y_{qpc} is an outcome of officer q in polling station p in sub-constituency c, and μ_c signifies sub-constituency fixed effects. Additionally included are fixed effects for the randomly assigned potential voter name, φ_n, and election officer type, π_v. $Match_{qpc}$ is an indicator variable taking value 1 if the election officer's group type and that of the potential voter are the same (e.g. Yadav and Yadav) and 0 otherwise. The potential-voter-name and officer-type fixed effects control for the average differences in assessed likelihood of the potential voter's ability to vote across the different hypothetical names and by officers of different types, so the coefficient of interest, θ, gives the average change in officer assessment caused by the officer-voter type match. Further controls included are fixed effects for polling team composition and a set of officer-level covariates: age, log monthly salary, an indicator for first term of service at a polling station, and fixed effects for occupation type, education level, and polling team position. A second specification additionally includes polling-station-level controls for log total registered voters, share Muslim/Yadav registered voters, and fixed effects for station location type and number of officer team members.

I consider as outcomes both an indicator variable taking value 1 if the officer indicates the individual would be "Likely" or "Very Likely" allowed to vote and a continuous variable taking the 1-to-4 scale value. Figure 1.5 shows that for both variables the average assessed

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without a photograph. He can recite his name and other particulars. On a scale of 1 to 4, how likely do you think it is that he would be allowed to cast a vote based on this information?", where the potential responses are "Very unlikely (1)", "Unlikely (2)", "Likely (3)", "Very likely (4)".

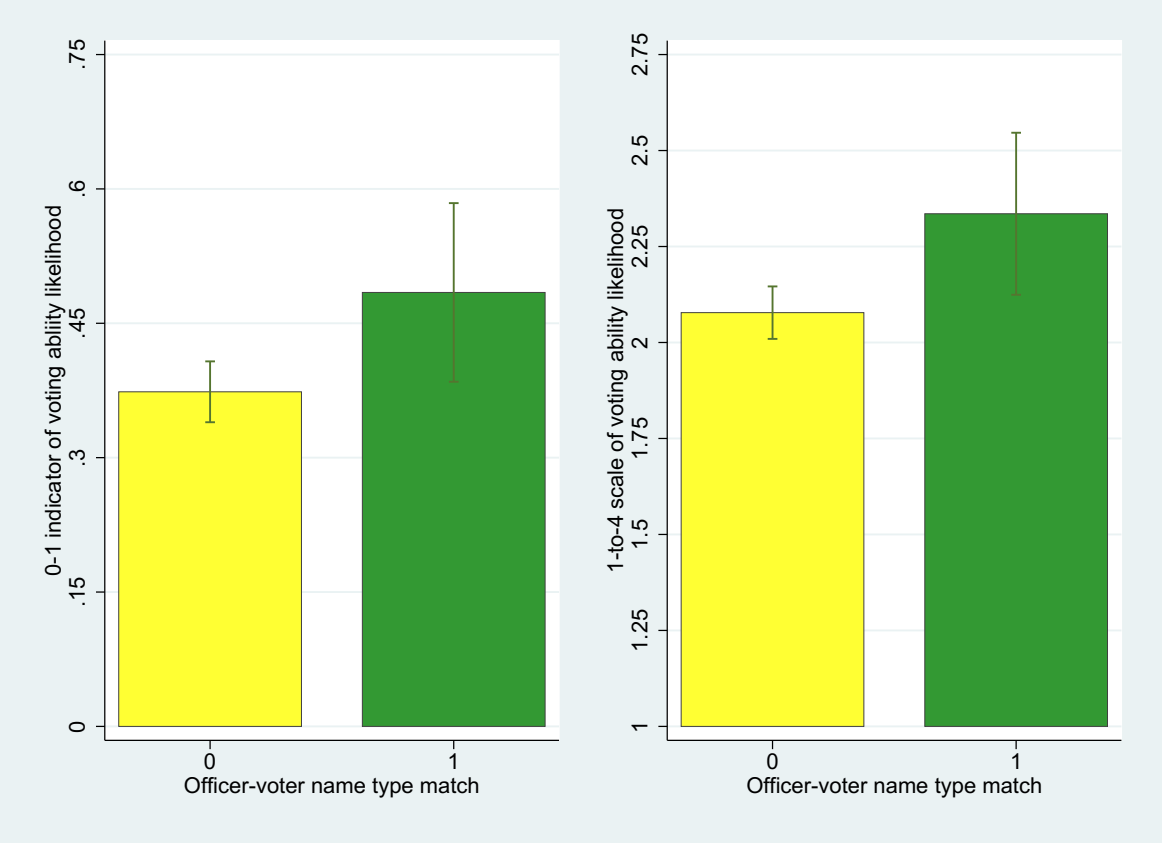
[23] Appendix Section A.2 provides the names used in each category.

Table 1.3: *Vignette experiment: own-type bias in officer assessment of voters*

	Ability to cast vote			
	0-1 indicator		4-point scale	
	(1)	(2)	(3)	(4)
Officer-potential voter name type match	0.103*	0.111**	0.237**	0.258**
	(0.056)	(0.055)	(0.117)	(0.116)
Observations	871	869	871	869
Name fixed effects	X	X	X	X
Officer type fixed effects	X	X	X	X
Individual controls	X	X	X	X
Polling station controls		X		X
Non-match group outcome mean [SD]	0.382	0.380	2.096	2.092
	[0.486]	[0.486]	[0.974]	[0.974]

Notes: Columns (1) and (2) report OLS estimates from regressions at the officer level of an indicator variable taking value 1 if the respondent answers "Very likely (4)" or "Likely (3)" as opposed to "Unlikely (2)" or "Very unlikely (1)" to the question: "A voter named [RANDOMLY ASSIGNED] arrives at the polling station without an EPIC card but has a government voter's slip without a photograph. He can recite his name and other particulars. On a scale of 1 to 4, how likely do you think it is that he would be allowed to cast a vote based on this information?" and 0 otherwise, on an indicator variable for whether the officer's own type matches that (Muslim, Yadav, Brahmin) of the randomly assigned voter name. Columns (3) and (4) report OLS estimates from regressions with the 1-4 scale value as the outcome. Columns (1) and (3) include fixed effects for respondent name and officer type, the stratification variables (sub-constituency in which officer was assigned to a polling station and officer category [Muslim/Yadav at mixed polling station, non-Muslim/Yadav at mixed polling station, non-Muslim/Yadav at homogeneous polling station] plus the following individual level controls: age, log monthly salary, an indicator for first term of service at a polling station, and fixed effects for occupation type, education level, and polling station position. Columns (2) and (4) further include polling station level controls for log total registered voters and proportion Muslim/Yadav registered voters, and fixed effects for station location type and number of team officers. Standard errors are clustered at the polling station level. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

likelihood of voting increases significantly when the hypothetical individual is of the same type as the election officer. Table 1.3 presents the underlying estimates from equation (1.3). Considering the binary outcome variable, columns (1) and (2) show a significant 10 percentage point, or more than 25 percent, increase in the probability that an individual is assessed as likely able to cast a vote. Similarly, using the 4-point-scale measure as the outcome in columns (3) and (4), a significant increase of approximately 0.24 points is observed.



Notes: The figure in the left panel depicts the estimated probabilities of an officer respondent indicating that a hypothetical individual described in the officer's survey vignette would be "(3) Likely" or "(4) Very Likely" able to cast a vote, as opposed to "(2) Unlikely" or "(1) Very Unlikely". The estimates are based on the regression in column (2) of Table 1.3, assuming mean values of all control variables. The left bar represents the randomly assigned subset of officer respondents for whom the hypothetical individual's type (Muslim, Yadav, Brahmin) did not match the officer's own type, while the right bar represents the subset for whom the types match. The figure in the right panel depicts the estimated 4-point scale values of a hypothetical individual's voting ability likelihood, based on the same question as the left panel. The estimates are based on the regression in column (4) of Table 1.3, assuming mean values of all control variables. The notes to Table 1.3 provide the full vignette question text. Error bars signify 95 percent confidence intervals.

Figure 1.5: *Own-type bias in officer assessment of voters*

1.6.2 List experiment: biased officer behavior on election day

I next consider whether biased officer behavior was perceived as a relevant election day phenomenon by voters and election officers in this setting. As direct elicitation of survey respondents may yield unreliable estimates of the occurrence of potentially sensitive topics such as biased officer behavior during elections, I employed list experiments in both the surveys of registered voters and election officials. This method of indirect elicitation has been used to generate measures of sensitive topics related to political and electoral behavior in a number of recent papers (Gonzalo-Ocantos 2010, Corstange 2012, Kramon and Weghorst 2012, Ahlquist et al. 2013, Burzstyn et al. 2014).

For each list experiment, respondents were randomly assigned to either a control or treatment group. Members of each group were asked to indicate only the total number of statements that occurred at their polling station during the 2014 elections from a list of statements read to them. Control respondents were given a list of four statements on non-sensitive election day topics, while treatment respondents were read the same list but with an additional sensitive statement included. This approach prevents individual-level determination of which statements were chosen, but allows for the population-level prevalence of the sensitive statement's occurrence to be estimated as follows:

$$N_{ipc} = \alpha_c + \phi Treat_{ipc} + \mathbf{X'_{ipc}}\lambda + \epsilon_{ipc}, \tag{1.4}$$

where N_{ipc} is the number of statements indicated as occurring at polling station p by respondent i, $Treat_{ipc}$ is an indicator variable for assignment to the group additionally receiving the sensitive statement, and $\mathbf{X_{ipc}}$ is a vector of polling station and individual characteristics. Assuming that respondents assess the sensitive item truthfully and the inclusion of the sensitive topic does not influence their evaluation of the non-sensitive items, ϕ gives an unbiased estimate of the population proportion for whom the sensitive item occurred. Standard errors are clustered at the polling station level. Additionally included are polling-station-level controls for log registered voters, share Muslim/Yadav registered voters and fixed effects for polling station number of officers and location type and for the

respective survey sampling strata. For officer-respondent regressions, individual controls for age, log monthly salary and fixed effects for occupation type, education level, first term of service, and team position are included. For voter-respondent regressions, controls for age, gender, log monthly household income, and household head status and fixed effects for occupation category and education level are included.

The survey of election officers included two list experiments.[24] The sensitive statements for the treatment group in the first and second experiments were: "One or more of the election officers at the polling station treated some voters differently based on the voters' religion or caste" and "One or more of the election officers tried to influence some voters' choice of candidate or make it harder for them to vote". Similarly, the two list experiments included in the survey of registered voters had the sensitive statements: "One or more of the election officers at your polling station treated you or others differently based on your religion or caste" and "One or more of the election officers at your polling station tried to influence how you or others voted or to make it more difficult for you or them to cast votes".

Table 1.4 presents the results of the list experiments. The estimates in column (3) of Panel A indicate that 19 percent of officers agree that at least one of the officers at their polling station treated voters differently based on religion or caste and 5 percent that at one least member of their polling station team tried to influence voter behavior, whether choice of candidate or making it more difficult to vote. I turn to the registered voter results in Panel B. Estimates imply that 25 percent of respondents indicate that election officials at their polling station treated voters differently based on religion or caste and 13 percent that election officers tried to influence voting behavior at their polling station.

The wording of these statements is admittedly somewhat broad, and respondents may vary in how they interpret them. Therefore, the aim of these experiments is not to provide precise estimates, but rather to demonstrate the occurrence of biased officer behavior connected to religion/caste on election day. The results, from both populations of

[24] Appendix Section A.2 provides the introductory prompt used in these experiments.

Table 1.4: *List experiments: biased officer behavior on election day*

	Control (1)	Treatment (2)	Difference (3)	Obs. (4)
Panel A. Election officers				
"One or more of the election officers at the polling station treated some voters differently based on the voters' religion or caste."	1.915 [0.777]	2.086 [0.873]	0.192*** (0.048)	878
"One or more of the election officers tried to influence some voters' choice of candidate or make it harder for them to vote."	2.883 [0.376]	2.960 [0.398]	0.047** (0.021)	877
Panel B. Registered voters				
"One or more of the election officers at your polling station treated you or others differently based on your religion or caste."	2.036 [0.758]	2.280 [0.913]	0.254*** (0.026)	3,532
"One or more of the election officers at your polling station tried to influence how you or others voted or to make it more difficult for you or them to cast votes."	2.396 [0.682]	2.539 [0.809]	0.128*** (0.023)	3,547

Notes: Columns (1) and (2) report unconditional means and standard deviations of the control (individuals receiving a list of four questions with the listed statement omitted) and treatment (individuals receiving a list of the same four questions plus the listed statement included). Column (3) reports the coefficient of an OLS regression at the individual level of the total number of statements the respondent indicated occurred at the polling station during the 2014 elections and sub-constituency fixed effects. Additionally included are polling-station-level controls for log total registered voters, share Muslim/Yadav registered voters, and fixed effects for location type and number of officers. In Panel A, additional officer-level controls are age and log monthly salary and fixed effects for occupation and education and controls for log monthly salary and prior election experience. In Panel B, additional registered-voter-level controls are fixed effects for education level and occupation type and controls for age, sex, household head status, and log monthly household income. Standard errors are clustered at the polling station level. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

respondents, suggest that officers do attempt to influence voting behavior on election day, and that religion and caste influence their treatment of voters. In the sections which follow, I conduct tests which allow for a further disentangling of mechanisms.

1.6.3 Election day experiences of potential voters

In this section I examine how the election day experiences of potential voters vary by officer team composition, both in terms of overall station area management and the individual-specific identity verification process. I use the following specification to test for impacts on the frequency of canvassing or disorderly behavior at the polling station, as reported by registered voter survey respondents:

$$Y_{wpc} = \mu_c + \theta_o + \beta Mixed_{pc} + \mathbf{X'_{wpc}}\lambda + \epsilon_{wpc} \tag{1.5}$$

where Y_{wpc} is an outcome for respondent w in polling station p in sub-constituency c. The included individual and polling station controls are the same as in equation (1.4). In an additional specification, I examine whether the reported impact of team composition differs with respondent type by interacting the officer team composition variable with an indicator for whether the respondent is Muslim or Yadav. Standard errors are clustered at the polling station level.

Columns (1) and (3) of Table 1.5 show that the likelihoods of canvassing and disorderly behavior are unaffected by team composition. Columns (2) and (4) further demonstrate that the absence of significant differences holds regardless of respondent type. These results suggest that stricter management of the area surrounding the polling station is not the primary channel through which a shift from homogeneous to mixed team composition impacts voting outcomes.

Turning to individual-specific experiences at the polling station, I employ the following regression:

$$Y_{wpc} = \alpha_{pc} + \phi MY_{wpc} + \lambda ID_{wpc} + \psi \left(MY_{wpc} * ID_{wpc}\right) + \mathbf{X'_{wpc}}\lambda + \epsilon_{wpc} \tag{1.6}$$

Table 1.5: *Overall polling station management*

	Canvassing at station		Disorderly behavior at station	
	(1)	(2)	(3)	(4)
Mixed team	0.005	0.002	-0.001	0.006
	(0.005)	(0.007)	(0.011)	(0.014)
Muslim/Yadav		-0.003		0.010
		(0.006)		(0.014)
Muslim/Yadav * Mixed team		0.006		-0.018
		(0.010)		(0.019)
Observations	3,733	3,733	3,775	3,775
Polling stations	351	351	351	351
Outcome mean [SD]	0.020	0.020	0.068	0.068
	[0.141]	[0.141]	[0.256]	[0.256]

Notes: All columns report OLS estimates from regressions at the individual level of the listed variable on an indicator for mixed officer team composition at the polling station. Even-numbered columns include an interaction with an indicator for whether the respondent is Muslim/Yadav. Additionally included in all columns are fixed effects for the stratification variables (sub-constituency and above-below district level median in terms of MY elector percentage) and individual-level controls for age, sex, education level, household head status, household structure type, occupation type, and log monthly household income. Polling-station-level controls are included for log total registered voters, share Muslim/Yadav registered voters, and fixed effects for the number of officers stationed at the polling station. Standard errors clustered at the polling station level. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

where α_{pc} are polling station fixed effects and ID_{wpc} is an indicator for voter identity card possession by individual w at polling station p. I estimate this regression separately for individuals in the randomly determined samples of mixed team and homogeneous team polling stations. The same set of registered-voter-level controls from equation (1.4) are included and standard errors are clustered at the polling station level.

I first consider a potential voter's ability to cast a vote as an outcome. I find in column (1) of Table 1.6 that, at polling stations with homogeneous officer teams, individuals are significantly less likely to be able to vote if they are Muslim/Yadav. This difference disappears among individuals with voter identity cards, which reduce the scope for officer discretion. In addition, for non-Muslim-Yadav individuals, possession of a voter identity card does not significantly change the likelihood of being able to cast a vote. Column (2) shows that, at polling stations with mixed officer teams, voter identity card possession significantly increases the likelihood of being allowed to cast a vote, but that this no longer varies with Muslim/Yadav identity. These regressions include a variety of individual-level controls, reducing concerns that the effects are driven by correlations between Muslim/Yadav identity or voter identity card posession with unobservables that influence voting ability. To summarize, at homogeneous team stations, voter identity cards matter in terms of voting ability only for Muslim/Yadav potential voters, while at mixed team stations they are important for potential voters of all types.

I subsequently examine as an outcome the likelihood of a potential voter having a satisfactory overall experience at the polling station on election day.[25] Column (3) shows that Muslim/Yadav potential voters facing homogeneous teams of officers rate their polling station experiences as worse on average, but only in the absence of voter identity cards. Non-Muslim/Yadav voters, however, express lower satisfaction if they possess a voter identity card. The latter effect could reflect that under-qualified non-Muslim/Yadav potential voters are relatively more appreciative of being allowed to vote than those with voter identity

[25]This variable takes value 1 if a respondent indicates that her overall voting experience at the polling station on election day was "Excellent","Good", or "Fair", as opposed to "Poor".

Table 1.6: *Identity verification experience of potential voters*

	Able to cast vote		Satisfactory overall station experience	
	Homog. team	Mixed team	Homog. team	Mixed team
	(1)	(2)	(3)	(4)
Muslim/Yadav	-0.104*	-0.047	-0.066+	0.001
	(0.062)	(0.066)	(0.041)	(0.031)
Possess voter identity card	0.011	0.105**	-0.019*	0.013
	(0.025)	(0.046)	(0.010)	(0.024)
Muslim/Yadav * Possess voter identity card	0.109*	0.033	0.072*	-0.004
	(0.062)	(0.066)	(0.043)	(0.033)
Observations	1,929	1,946	1,907	1,900
Polling stations	175	176	175	176
Outcome mean [SD]	0.981	0.980	0.981	0.982
	[0.137]	[0.138]	[0.136]	[0.133]

Notes: All columns report OLS estimates from regressions at the individual level of the listed variable on an interaction of the Muslim-Yadav respondent indicator with an indicator for voter identity card possession, for the sample of polling stations indicated in each column. Additionally included are polling station-level fixed effects and individual-level controls for age, gender, education level, household head status, household structure type, occupation type, and log monthly household income. "Satisfactory overall station experience" is an indicator for whether the respondent indicated that their overall voting experience at the polling station on election day was "Excellent"/"Good"/"Fair", as opposed to "Poor". Standard errors clustered at the polling station level. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent. + p-value = 0.112.

cards, who are more certain that they should be allowed to do so. I find in column (4) that these impacts are absent at mixed team polling stations. Overall, the results in Table 1.6 are consistent with mixed team composition and voter identity card provision each reducing the differential treatment of potential voters at polling stations, where homogeneous teams are relatively more stringent toward Muslim/Yadavs.

1.6.4 Heterogeneity in effects by voter identity card coverage

Using experimental and survey data, the previous sections established that election officers are relatively biased in favor of potential voters of their own type and that individuals' religious and caste identities can influence their ability to vote if they do not have a voter identity card. If in general identity card possession reduces the scope of potentially discriminatory discretion available to officers and mixed team composition shifts station administration of the voter identification process to be more neutral, a substitute relationship in the impacts of the two on polling-station-level voting outcomes would also be expected. Returning to the polling station administrative data on voting outcomes, I test for this substitutability using specifications of the form:

$$Y_{pc} = \mu_c + \theta_o + \beta Mixed_{pc} + \eta(Mixed_{pc} * ID_c) + \mathbf{X'_{pc}}\lambda + \epsilon_{pc,} \tag{1.7}$$

where ID_c is the proportion of registered voters in sub-constituency c without a voter identity card.[26] Polling-station-level controls included are the log number of registered voters and the Muslim/Yadav share of registered voters. The top one percent of observations in terms of the absolute value of the vote share margin between the RJD and BJP are trimmed.[27] The main effect for ID_c is absorbed by the sub-constituency-level fixed effects, and the coefficient of interest is η, where an estimated sign opposite that of β indicates that polling station composition and voter identity card coverage exhibit substitutability in their impacts on voting outcomes. Sub-constituency-level voter identity card coverage is not randomly

[26]The lowest level at which data on voter identity card coverage is available from the government.

[27]These are polling stations where one coalition won by a margin of at least 88 percent.

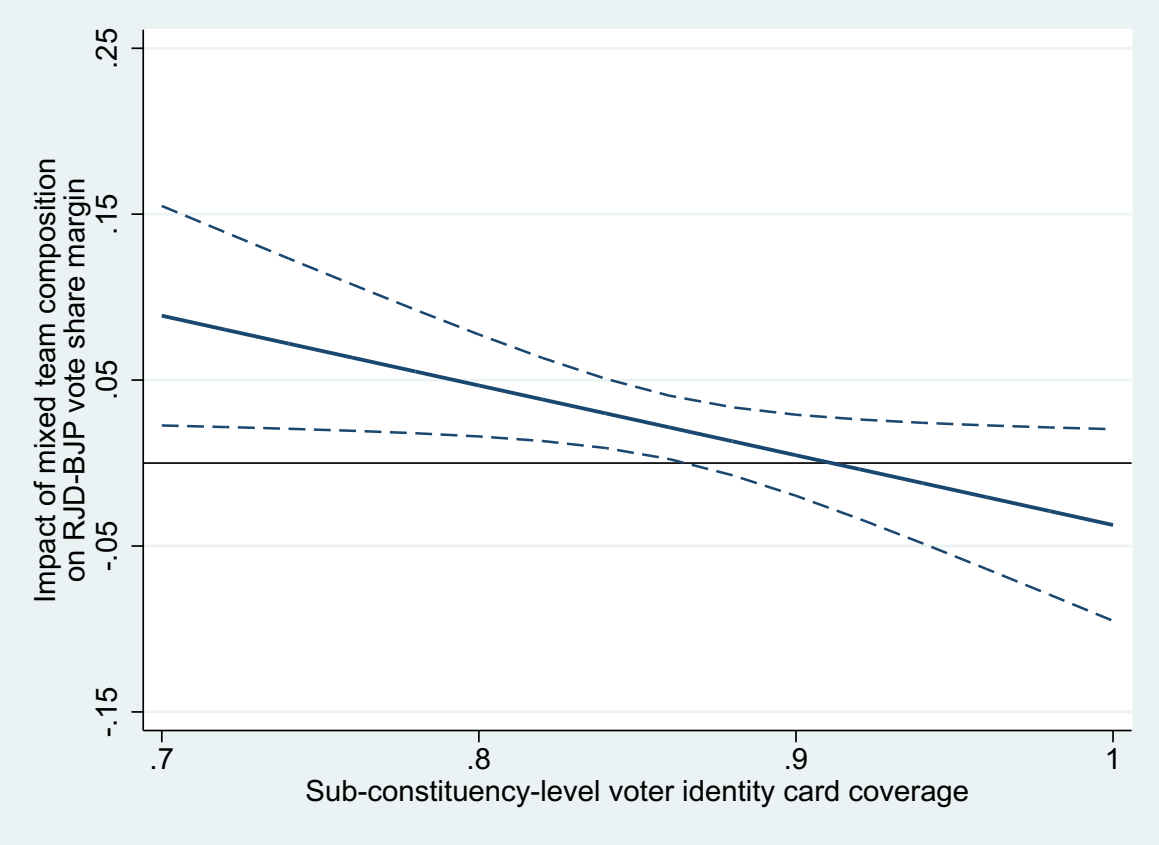
Table 1.7: *Heterogeneity in effects of team composition by voter identity card coverage*

	Ln votes RJD		Ln votes BJP		Vote share margin RJD-BJP	
	(1)	(2)	(3)	(4)	(5)	(6)
Mixed team *	-0.009*	-0.014*	0.005	0.010*	-0.004**	-0.006**
Voter identity card coverage %	(0.005)	(0.008)	(0.004)	(0.006)	(0.002)	(0.003)
Mixed team	0.809	1.072	-0.453	-0.960*	0.383**	0.558*
	(0.457)	(0.768)	(0.332)	(0.579)	(0.173)	(0.314)
Observations	5,429	5,429	5,439	5,439	5,442	5,442
Polling station controls	X	X	X	X	X	X
Sub-constituency fixed effects	X	X	X	X	X	X
Interacted sub-constituency controls		X		X		X
Implied effect: minimum sample	0.129**	0.152***	-0.079**	-0.086**	0.063***	0.072***
card coverage sub-constituency	(0.054)	(0.057)	(0.040)	(0.043)	(0.022)	(0.024)

Notes: All columns report OLS estimates from regressions at the polling station level of the listed variable on an indicator for mixed team composition interacted with the sub-constituency-level percentage of registered voters with a voter ID card. Also included are sub-constituency and number of officer fixed effects and controls for Muslim/Yadav share of registered voters and log total registered voters. Even-numbered columns additionally include interactions (not shown) with sub-constituency-level measures of the population proportions that are literate and Schedule Caste/Schedule Tribe, and the share of registered voters that are Muslim/Yadav (none of these interaction effects are statistically significant). The implied effect given in each column reflects the estimated impact of mixed team composition for the sub-constituency with the lowest level of voter identity card coverage observed in the sample. Coverage ranges between 76.3 and 93.9 percent in sample sub-constituencies. The sample trims the top one percent of observations in terms of absolute value of coalition vote share margin (polling stations with a margin greater than 88 percentage points). *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

determined, and may be correlated with other characteristics that mediate the impact of team composition on voting outcomes. As a robustness check I therefore consider a specification where I additionally interact officer team composition with sub-constituency-level measures of the population proportions that are literate, Scheduled Caste/Scheduled Tribe, and Muslim/Yadav.

In columns (1) and (2) of Table 1.7, I find that the positive impact of mixed team composition on RJD votes decreases by a significant 0.9 percentage points per 1 percentage point increase in voter identity card possession. The results for BJP votes in columns (3) and (4) also indicate that the team composition effects are strongest in areas with low voter identity coverage. Columns (5) and (6) show that the vote share margin shift toward the RJD caused by changing from a homogeneous to a mixed team is approximately 0.5 percentage points smaller per 1 percentage point increase in voter identity card coverage.



Notes: Figure plots the estimated polling-station-level impact of mixed team composition on the vote share margin between the RJD and BJP coalitions at different levels of sub-constituency-level voter identity card coverage. Dashed lines signify 95 percent confidence intervals. Calculated using the estimates from Column (5) of Table 1.7.

Figure 1.6: *Heterogeneity by voter identity card coverage in impact of team composition*

Voter identity card coverage in my sample of sub-constituencies ranges from 76.3 to 93.9 percent. Figure 1.6 plots the implied effect of mixed team composition over a similar range of voter identity coverage and demonstrates that the significant impact observed at low coverage levels becomes insignificant as full coverage is approached. These estimates, taken together with the earlier experimental and survey results, provide strong evidence that mixed team composition and identity card coverage serve substitute roles in preventing officer bias from undermining the neutrality of the identity verification process for potential voters.

1.7 Alternative explanations

A possible concern in attributing the previously identified impacts to bias associated with officers' religious and caste identities is that there may exist other characteristics that correlate with these identities and also influence voting outcomes. This is unlikely to explain the above results for two reasons: the previous analysis captures the effects of the presence on otherwise homogeneous teams of officers that are either Muslim or Yadav, two groups which are not particularly similar outside of their political alliance; and indviduals of different religions and castes serving as polling station officers are more likely to be similar along other dimensions than would be their populations in general.

First, Yadavs are a lower-caste Hindu group in Bihar and, other than in political orientation, it is unclear along what dimensions they would be systematically more similar to Muslims than to other Hindu groups, especially given the dispersed support for the BJP across upper- and lower-castes in these elections.[28] In Appendix Table A.6, I examine the influence of Muslim and Yadav officer presence separately using a regression specification analogous to that of equation (1.1). The estimates across columns (1) through (4) reveal similar impacts for Muslim and Yadav officers. The coefficients for the two groups are statistically indistinguishable in each case, and the shift in vote share margin toward the RJD is significant at the 5 percent level for both Muslim and Yadav presence on officer teams.

Second, polling station officers are selected from pools of government employees who are likely more similar than would be average individuals from different religious and caste groups. I explicitly test for differences by Muslim/Yadav status in the sample of surveyed polling station officers across a number of characteristics proxying for experience and knowledge: age, log monthly salary, college graduation, and prior election officer experience. I regress each of these outcomes on an indicator variable for Muslim/Yadav identity and fixed effects for sub-constituency and team position. As a further check, I also

[28] Highlighting the differences between the two groups, Lalu Prasad Yadav, the politician responsible for the creation of the Muslim/Yadav coalition, has even said "I have made an alliance between those who worship the cow [Yadavs] and those who eat the cow [Muslims]." (Wittsoe 2013, p.60)

Table 1.8: *Variation in other officer characteristics by Muslim/Yadav identity*

	Survey data				Administrative data	
	Age	Ln monthly salary	College graduate	First time officer	Age	Ln monthly salary
	(1)	(2)	(3)	(4)	(5)	(6)
Muslim/Yadav officer	-0.340	0.018	-0.025	-0.019	0.439	0.001
	(0.554)	(0.029)	(0.030)	(0.031)	(0.373)	(0.012)
Observations	912	888	911	903	5,983	6,198
Non-Muslim/Yadav outcome mean [SD]	42.822 [9.727]	9.563 [0.584]	0.684 [0.465]	0.333 [0.472]	44.975 [9.802]	9.291 [0.363]

Notes: All columns report OLS estimates from regressions at the officer level of the listed variable on an indicator for Muslim/Yadav identity. Additionally included are sub-constituency and officer-position fixed effects. Columns (1) through (4) are based on reported data from the survey of officers. Columns (5) and (6) are based on full sample of administrative data available for the same district in which the surveys were conducted. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

construct measures of age and log monthly salary based on separate administrative data available for the full population of election officers in the district in which the officer survey was conducted. The results in columns (1) through (6) of Table 1.8 show that in no case are there significant differences by Muslim/Yadav status.

1.8 Conclusion

Having identified significant impacts of officer team composition on voting outcomes within and across polling stations, a natural question is whether changes in composition influence who ultimately wins elections. To examine this possibility, I conduct counterfactual calculations of the effects of alternative officer assignment mechanisms on the identities of winners in the 2014 parliamentary elections in Bihar.

I first use administrative data available across the state of Bihar to calculate the sub-constituency-level average numbers of neighbor polling stations. Second, the observed margins of victory from these elections already reflect the effects of the underlying (but unobserved outside of the two study districts) proportions of mixed team polling stations

in each parliamentary constituency. Finally, I assume that the proportion of mixed team polling stations in each sub-constituency is the same as the average value (0.324) across the two districts for which it can be directly observed in my data. I can then calculate the magnitudes of the shifts in the proportions of homogeneous and mixed team polling stations required to change the outcome of each election in which the RJD and BJP coalitions were both either winner or runner up.[29]

I use these magnitudes to consider the effects of two alternatives to the current method of randomized officer assignment: (1) requiring mixed team composition in all polling officer teams, and (2) excluding Muslim/Yadav officers from teams. During the 2014 elections, the RJD and BJP fielded the top two candidates in 29 of the 40 parliamentary constituencies in Bihar (Appendix Figure A.4 provides the distribution of vote share margins). As shown in Table 1.9, a shift to Alternative 1 is estimated to switch one election outcome in favor of the RJD and a shift to Alternative 2 to change one outcome to a BJP victory. I repeat this exercise for the most recent prior state assembly elections in 2010, where the RJD and BJP fielded the top two candidates in 185 of 243 races. Reflecting the lower levels of voter identity card coverage and greater number of close contests, thirty-three races are estimated to change to an RJD victory under Alternative 1 and six elections to switch in favor of the BJP under Alternative 2, or a combined 16 percent of the total.

In addition, the religious composition of candidates put forward in elections differs considerably across the coalitions; 17.5 percent of RJD coalition candidates in the 2014 Bihar elections were Muslim, as compared to just 2.5 percent for the BJP coalition. Accounting for the religious identities of candidates, the previous counterfactual calculations also indicate that a shift to all mixed team polling stations in Bihar would have led to a 25 percent increase in Muslim legislators both in the 2010 assembly and 2014 parliamentary elections. Recent work has shown that increasing Muslim representation in state legislatures in India results in significant reductions in child mortality rates and gains in educational attainment

[29] Appendix Section A.3 additional details. The vote share margin between the runner-up candidate and the remainder of the field is generally large enough that having a third place or lower candidate shift to become the winner could not feasibly occur as a result of changes in team composition.

Table 1.9: *Changes in election outcomes under alternative officer assignment mechanisms*

	Alternative 1: All mixed teams		Alternative 2: No mixed teams			
	BJP to RJD victory (1)	Vote share margin range (2)	RJD to BJP victory (3)	Vote share margin range (4)	RJD/ BJP top two parties (5)	Total races (6)
National parliament, 2014	1	-0.024	1	0.010	29	40
State assembly elections, 2010	33	[-0.066, -0.0003]	6	[0.004, 0.023]	185	243

Notes: This table reports estimates of the potential number of races for which the winning candidate would have switched between the RJD coalition and the BJP coalition, under two alternative officer assignment scenarios. Alternative 1 is the presence of all mixed composition teams and Alternative 2 is the absence of any mixed composition officer teams, assuming an initial 0.324 proportion of mixed teams (that observed in the available 2014 data). Columns (1) and (3) give the number of races for which the winning party would change as indicated. Columns (2) and (4) give the range of the RJD-BJP coalition vote share margins observed in the impacted constituencies. Column (5) gives the number of races in which the RJD and BJP coalitions fielded the top two candidates, and column (6) the total number of races in Bihar for that election cycle. The calculation accounts for spillover effects from neighboring mixed team polling stations and heterogeneity in impact by voter identity card coverage (at the sub-constituency level).

across both Muslim and non-Muslim households (Bhalotra et al. 2014). This suggests that the impacts on election outcomes associated with officer team composition may also have important downstream effects on outcomes directly relevant to citizen well-being.[30]

Fair and well-functioning elections are critical to maintaining the responsiveness of elected officials to citizens in democracies. While the related literature on election reforms has focused in large part on the benefits of advances in monitoring and voting technology, this paper is to my knowledge the first to provide rigorous evidence of the remaining importance of the identities of local-level election personnel. Indian elections are technologically advanced and their administration is highly regulated, indicating that bias in discretionary decision making of polling station officers can undermine the quality of service provision

[30]It is also possible that the effects of officer team composition on citizens' election day experiences influence their expectations and behavior in subsequent elections, for instance in whether to turn out at the polling station. I use polling-station-level data for the 2015 state assembly elections in Bihar and find in Appendix Table A.7 that voting outcomes in these elections are unaffected by officer team composition from 2014, providing no evidence of cross-election-cycle impacts on voter behavior.

even at the present frontier of election practice.

Though my findings suggest that diversity within teams of election officers can improve the impartiality of polling station management, it may not always be politically or administratively feasible to mandate that such mixed composition occur. It could also be difficult in other contexts to determine the relevant dimensions of identity along which diversity should be defined. My results, however, additionally demonstrate that policies which reduce the scope for officer discretion in the first place, such as the widespread provision of voter identity cards, may be promising alternatives in reducing the ability of local-level election officials to influence voting outcomes. More generally, the findings of this paper demonstrate that institutions which require greater discretionary decision making by bureaucrats or other government employees may be more susceptible to adverse impacts of these individuals' underlying biases on the quality of public services.

Chapter 2

Can Electronic Procurement Improve Infrastructure Provision? Evidence from Public Works in India and Indonesia[1]

2.1 Introduction

Contestable government procurement of goods and services has been estimated at over 7 percent of world GDP (OECD 2002), with the incidence rising in emerging economies: For instance, in 2007, the Indian government spent over US $21 billion ($18 per capita) on the procurement of external goods and services, over double what it spent in 2000 (IMF). Yet, both the quantity and quality of recently constructed public infrastructure often remain low (Briceño-Garmendia, Estache, and Shafik 2004). A limited supply of local qualified contractors, collusion among contractors, and corruption among public officials have each been cited as important reasons (Kenny 2007).

[1]Co-authored with Sean Lewis-Faupel, Benjamin A. Olken, and Rohini Pande

A number of governments have responded by adopting electronic procurement (henceforth: e-procurement) (World Bank 2007). Broadly, e-procurement is the implementation of a technological platform as directed by a potential buyer (a government agency or firm) to facilitate transactions between that buyer and potential sellers of goods and services. Commonly, the practice includes electronic postings by the buyer of products and services desired for procurement. There is often an online method for potential suppliers to offer those goods or services requested by the buyer, under a contract and price either preordained by the buyer or offered by the potential seller. As with traditional procurement, there is typically a method for choosing among contracts or prices offered by the potential sellers. In some cases, this selection process is performed automatically by the technology.[2] In other cases, e-procurement consists of contracted sellers providing a set of goods or services at fixed prices from which the buyer can choose as needed. Additionally, e-procurement may allow for the electronic transfer of funds as agreed upon in the procurement contract. Finally, the technology can facilitate a review by the buyer of the goods or services delivered by the contracted seller, which may be communicated to the seller, considered when enforcing contracts, and used by the buyer in future procurement decisions. We describe the specifics of the e-procurement reforms in the Indian and Indonesian contexts in the next section.

E-procurement can potentially address three common concerns with manual procurement practices: lack of access to bid information, collusion among bidders, and corruption. By lowering the costs of obtaining information about a tender process, e-procurement may increase the number of firms who can bid. Likewise, it can reduce bidder collusion by providing tender information to firms outside a local cartel, allowing non-cartel firms to participate and breaking up local bidding cartels. E-procurement can also mitigate corruption by reducing government officials' ability to selectively withhold information or refuse electronic bids from non-favored bidders. Moreover, by ensuring public access to

[2]The system may also allow potential sellers to electronically submit other supporting documentation, such as resumes or technical capabilities, or allow the buyer and potential sellers to communicate electronically. Additionally, e-procurement may be used to record and archive any number of other data points of interest to the buyer.

all procurement data, e-procurement enhances transparency and the possibility of public oversight.

However, it is plausible that in low income settings, where information technology coverage and other aspects of state capacity remain low, e-procurement can only effect limited change and can potentially make things worse. Potential contractors (who are currently not in the system) may continue never to learn about available tenders, and cartels and corrupt officials may continue to use strong-arm tactics to prevent entry by such contractors. If many small firms have limited access to the internet, requiring electronic bids could harm competition.

In this paper we examine the impact of electronic procurement on public works projects in two large emerging economies: India and Indonesia. In India, we examine procurement practices between 2000 and 2009 for a federally funded rural road construction program which is implemented by state road departments, the *Pradhan Mantri Gram Sarak Yojana* (PMGSY) program. Under this program, roughly 145 road packages were tendered per state per year. In Indonesia, we examine contract data from the national Ministry of Public Works for both construction and consulting (e.g., engineering management and design) contracts each year. On average, 32 consultancy and 58 construction packages per province were issued each year. The gradual roll-out of e-procurement (at the state-level in India and province-level in Indonesia) allows for a difference-in-differences strategy: We compare outcomes in states/provinces before and after the adoption of e-procurement, as well as in those continuing under manual procurement practices, allowing us to quantify the benefits or costs of the practice in both countries.

For both countries, we obtained administrative data on the complete universe of contracts from before and after e-procurement by scraping publicly available information from respective government websites. In Indonesia we have bidding and final contract data for all tenders; in India, the website publishes final contract data but not the details on individual bids. All told, this leaves us with a dataset of over 20,000 contracts in India and over 14,000 tenders in Indonesia. In addition, in India, we hand collected bidding data on tenders for

four states which we use to supplement the administrative data.

We first show that, in both India and Indonesia, e-procurement increases the probability that the winning bidder comes from outside the region where the contract takes place. This is consistent with e-procurement decreasing the costs of submitting bids for those not physically present. We next examine the impact on the ultimate outcomes of interest: price, quality of construction, and timeliness. We find no systematic evidence that electronic procurement lowers prices paid by the government. In Indonesia, the point estimates are consistent with small (2-5 percent) reductions in prices, but these are not statistically significant. In India, final prices are unaffected. Overall, we can statistically rule out (at the 5 percent level) declines in contract values of more than 2.7 percent in India, of more than 6.6 percent for Indonesian consulting projects, and of more than 14.1 percent for Indonesian works contracts.

In contrast, e-procurement led to quality improvements, albeit along different dimensions in the two countries. A first measure of quality is time-overrun in project completion. Reports of corruption of procurement in India typically focus on cases where works are abandoned halfway through or completed in a very tardy manner. In our data we see that 77 percent of road projects in India and around 95 percent of public works projects in Indonesia are completed late. In India, we observe no statistically significant changes in late works, while in Indonesia these declines are large and significant – whereas only 5 percent of conventionally procured construction projects in Indonesia are completed on schedule, 20 percent of electronically procured construction projects are on time.

A separate indicator of quality, only available for India, is an independent audit report on construction quality, which was conducted identically in roads completed under both e-procurement and traditional procurement. According to this measure, we find that e-procurement leads to higher quality roads, with the quality grades rising by about 12 percent in e-procurement projects compared to other projects.

We then explore the degree to which the results are driven by improving outcomes among already winning bidders, as opposed to changing who wins. To see whether the

observed changes in outcomes reflect changes in selection of who wins, for the key variables of interest – price, delays, and quality – we estimate a fixed effect for each contractor and examine how e-procurement changes the average quality characteristics of winning contractors (i.e., the fixed effects of who wins). We find that after e-procurement, winning contractors in India tend to be those who have higher quality on average. In Indonesia, we find evidence that those contractors who win after e-procurement are systematically less likely to be late. This suggests that a key mechanism for e-procurement is allowing higher quality contractors to enter and win projects, rather than simply encouraging better performance from an existing set of contractors. Increased entry of contractors could reflect better information flows and/or reduced ability of local contractors to prevent others from filing tenders.

This paper is related to several economic literatures. While there is a growing body of work which examines procurement (Bandiera, Prat, and Valletti 2009, Krasnokutsaya and Seim 2011), much of the literature has focused on the award procedures (i.e., scoring auctions vs. lowest-price auctions, etc.) (Tran 2008) and potential interactions with other procurement regulations (Decarolis 2014). This paper suggests that an important component, at least for ensuring quality, can be in the implementation of the procurement auction, holding these rules fixed. It also contributes to a growing literature using procurement data to examine corruption (Di Tella and Shargrodsky 2003, Ferraz and Finan 2008, Bandeira, Prat, and Villetti 2009, Bobonis, Fuertes, and Schwabe 2010, Cai, Henderson, and Zhang 2013).

Second, past work has highlighted the role of media and technology in shaping the political and policy landscape. Strömberg (2004) suggests that media access among constituents may make politicians more responsive to voters, while Falck, Gold, and Heblich (2012), Jaber (2013) and Campante, Durante, and Sobbrio (2014) measure causal impacts of internet access on political participation. Our paper complements this previous literature by considering how new technology, in this case implemented by government itself, may improve the outcomes of a government initiative.

Finally, this paper is part of a recent, broader agenda that documents the role of

communication technology in development. While there are several studies that document the impact of cell phone technology on market access (Jensen 2007, Aker 2010) and education (Aker, Ksoll, and Lybbert 2012), this paper represents one of the first studies to examine the impact of the internet on governance issues.

One advantage of our approach is that we use the same methodology to study separate e-procurement programs in two different countries. To the extent that the findings from both countries are similar, the external validity of our results is given credence. To the best of our knowledge, this study represents one of the first microeconomic studies that uses difference-in-differences to simultaneously evaluate a new program in multiple countries, allowing for more careful conclusions with respect to external validity.[3]

The paper is structured as follows: In Section 2.2 we describe the institutional details relating to procurement practices and road construction in India and Indonesia. In Section 2.3 we describe the data and empirical strategy. In Section 2.4 we report the findings. Section 2.5 concludes.

2.2 Background

We start by describing the public works programs in our two study countries, followed by the nature of e-procurement adopted in these two settings. We conclude the section with a brief description of likely channels of influence of e-procurement.

2.2.1 India

Public Works Program and Manual Procurement

In the year 2000, India launched a large-scale rural road construction scheme called *Pradhan Mantri Gram Sarak Yojana* (PMGSY). The federal government provides funding for this scheme and coordinates program implementation, but full executional responsibility lies

[3]The other study of this nature we are aware of is Gruber and Mullainathan (2005), which evaluates the impact of state and provincial cigarette tax changes in the US and Canada.

with state governments. Each state has a rural roads department which decides the schedule for road construction and manages procurement. PMGSY roads follow a uniform criteria for road construction in terms of material usage and quality for all Indian states.

For PMGSY, all states use the same procurement rules and standardized bidding document, provided by the national roads agency. The procurement process follows a cost-based auction procedure. Specifically, conditional on meeting a pre-specified set of technical qualifications intended to ensure a contractor is capable of completing the project, the contract is awarded to the lowest bidder. Importantly, these rules are identical for manual and electronic procurement.

Anecdotally, public procurement in India is rife with corruption, and claims of impropriety exist throughout the contracting process. Contractors have reported being physically intimidated or barred from submitting bidding documents. The handling of bidding documents after submission has also been called into question with claims of altered bids, inspection of bids prior to technical reviews, and intentional loss of submissions. There is also concern that technical qualifications are used to unreasonably exclude certain firms from the bidding process. The implication of many of these reports is that government officials collect rents in exchange for some advantage in the bidding process.

As a case study, we examined the tendering process for manual procurement for a random sample of 188 road contracts issued between 2001 and 2005 in the Indian state of Uttar Pradesh. As Figure 2.1 shows, there is very little competition in PMGSY contracts. In 95 percent of cases, the price bid of only one firm was evaluated; that is, there was only one bid submitted or all other bids were disqualified based on technical requirements. When we observe multiple bids here, over three quarters of the time all but one bid are disqualified. In the case of any technical disqualification, all but one bidder are disqualified 100 percent of the time. Prima facie this pattern of disqualifications is consistent with corrupt officials enforcing a desired winner.

In 2000, when the program began, all Indian states used a manual paper-based procurement system to bid out contracts to private contractors. This process involved obtaining

Number of bidders	Number of technical disqualifications				
	0	1	2	3	4
1	155				
2	5	14			
3	1		5		
4	3			4	
5					1
Total	164	14	5	4	1

Notes: Figure presents bidding data from a random sample of 188 road contracts issued between 2001 and 2005 in the Indian state of Uttar Pradesh (prior to e-procurement). Each cell is a count of tenders with the respective number of bidders and technical disqualifications. Technical disqualifications are intended to prevent a bidder who lacks the necessary expertise or equipment from competing. Blank cells indicate zeros. Note that the diagonal comprises all cases of all-but-one disqualified.

Figure 2.1: *Number of contracts by numbers of technical disqualifications and bidders*

internal approval of the project, publishing a Notice Inviting Tenders (NIT) in several media outlets (typically newspapers), having suppliers obtain detailed bid preparation materials from the government, receiving bid submissions from suppliers, receiving bid evaluations by buyers, and finally, the awarding of the procurement order and signing of agreements. The complete process required a long chain of internal authorizations (at times involving several departments), several visits by suppliers to departments, and the generation of reams of paper-based statements and evaluations.

E-Procurement

The Indian IT Act of 2000 provided legal recognition to electronic transactions. Since then, several Indian states have passed legislation enacting e-procurement, and the rural roads department in several states followed by adopting e-procurement practices for road construction. The implementation of e-procurement for PMGSY typically involves placing all invitations for tender into an online, searchable database. An online portal is also established that allows authorized users (contractors) to upload bidding materials. The Indian e-procurement systems prohibit procurement officials from viewing contractor names and viewing or editing price bids before completing the technical certification process, in

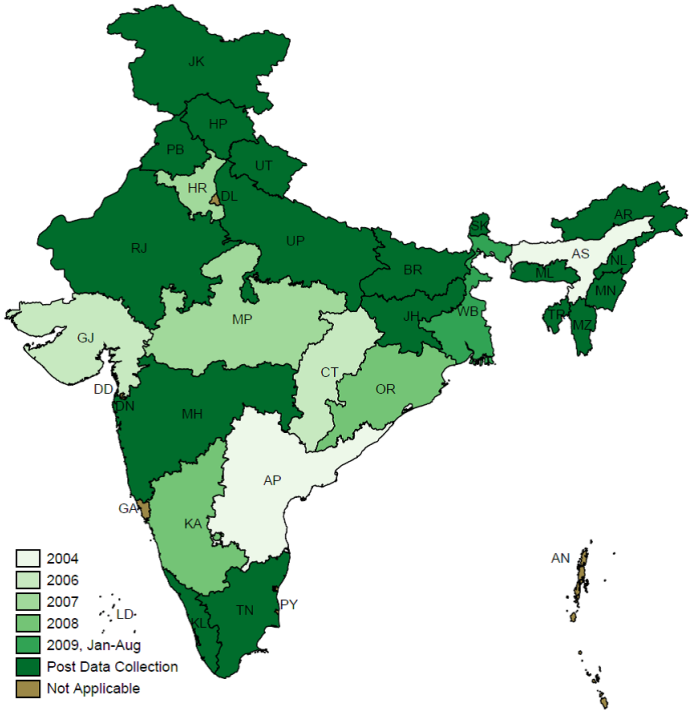


Figure 2.2: *E-procurement adoption - India*

theory preventing favoritism in the selection process. Given the above description of how most bidders are disqualified during the technical stage, this is potentially important for preventing corrupt practices. Finally, much of the electronic data is stored after auctions are completed, creating the potential for accountability.

We exploit this variation across time and states for our empirical analysis. Our sample covers 27 Indian states and territories during the period January 2000 through August 2009. During this time frame, 9 states adopted a system of electronic procurement. Figure 2.2 shows the dates when e-procurement rules were adopted in various states through August 2009. The first state to adopt, in 2004, was Andhra Pradesh, which (along with Karnataka) is one of the technological leaders of India. (Note that in some states (Chattisgarh, Gujarat, and Orissa), contracts which are below a state-specified cost threshold do not go through the electronic procurement process and are processed on paper. Since the threshold may respond endogenously to the auction policy (see, e.g., Tran 2008), during the applicable periods, we classify all projects as electronically tendered in these states.)

2.2.2 Indonesia

Public Works Program and Manual Procurement

In Indonesia, we examine the national Ministry of Public Works (MPW). The MPW procurement process covers mainly two types of contracts, works and consulting. Works contracts refer to projects such as the building or repairing of roads and bridges, where physical services or construction work are conducted by the contract winner. Access to heavy equipment and various construction materials is therefore typically a necessary component for these projects. Consulting contracts, in contrast, entail the provision of professional expertise by the winning bidder in planning or supervision services such as design and management. Depending on the project, procurement takes place either at the national headquarters in Jakarta or at the provincial offices of the MPW.

Works and consulting contracts additionally differ in the structure of their bidding processes. Works contracts are generally assigned to the low bidder conditional on meeting minimum administrative and technical qualifications, as in India. Consulting contracts involve a pre-qualification phase in which potential bidders first submit a document containing administrative, financial, and work experience information. A shortlist of firms meeting minimum qualification requirements are then invited to submit technical and cost bids. The winning bidder is assigned using a formula based on a combination of technical score and price, rather than simply lowest bid conditional on meeting minimum technical requirements as with works projects. For both types of contracts, a minimum of three bid submissions are required; otherwise, the processes are repeated. The overall pool of bidding firms consists of both private firms and state-owned construction firms, with state-owned firms competing against the private sector without any special preferences.

Public procurement is widely considered "one of the most corruption-ridden sectors" (Freedom House 2012) in Indonesia. For example, the 2009 Enterprise Survey of Indonesia (World Bank and IFC) sampled 1,444 firms comprising a representative sample of the non-agricultural formal private economy in the country. In the survey, 38.1 percent of respondents who had attempted to secure a government contract in the previous year indicated that firms

with characteristics similar to theirs make informal payments or give gifts to public officials to secure such contracts, though the average value given for these bribes was only 1.8 percent of the total contract value. A separate survey of 792 randomly sampled construction, consulting, and supplier firms conducted in 2010 by the Indonesia Procurement Watch (Indonesia Procurement Watch 2011) provides further suggestive evidence of corruption in the government procurement process, with 92.7 percent of respondents answering that they thought their firm had ever given bribes to the government officials involved in managing procurement. Additionally, 97.3 percent of respondents believed that it was not possible to win the contract tender without bribery and more than 95 percent indicated that the typical value of a bribe was more than 10 percent of the contract value. Beyond survey results, of the 196 cases considered by the Corruption Eradication Commission (KPK) of the Government of Indonesia between 2004 and 2010, 86 dealt with bribery and graft related to the procurement of goods and services and every such case resulted in a conviction (Onishi 2009, Parlina 2011).

Anecdotal evidence suggests that the manual procurement process contributes to the corruption problem in much the same ways as discussed in India. For example, government officials may not make the detailed documents required to prepare a bid available to non-favored firms or purposely misinform them about the proper submission process (UNAFEI 2008). Alternatively, there could be physical intimidation of firms that are not part of the cartel from submitting bids.

E-Procurement

Indonesia began rolling out a "semi-electronic procurement" (SEP) system in 2004 in the central ministry in Jakarta and expanded the procedure across the 33 provincial offices in approximately concentric circles from Jakarta over the next 5 years, as shown in Figure 2.3. The roll-out plan was devised from Jakarta and followed a standard pattern in Indonesia: roll-out started in Java, spread to the most developed provinces in Sumatra and Sulawesi next, and then was progressively rolled out throughout the country. Under SEP, firms are

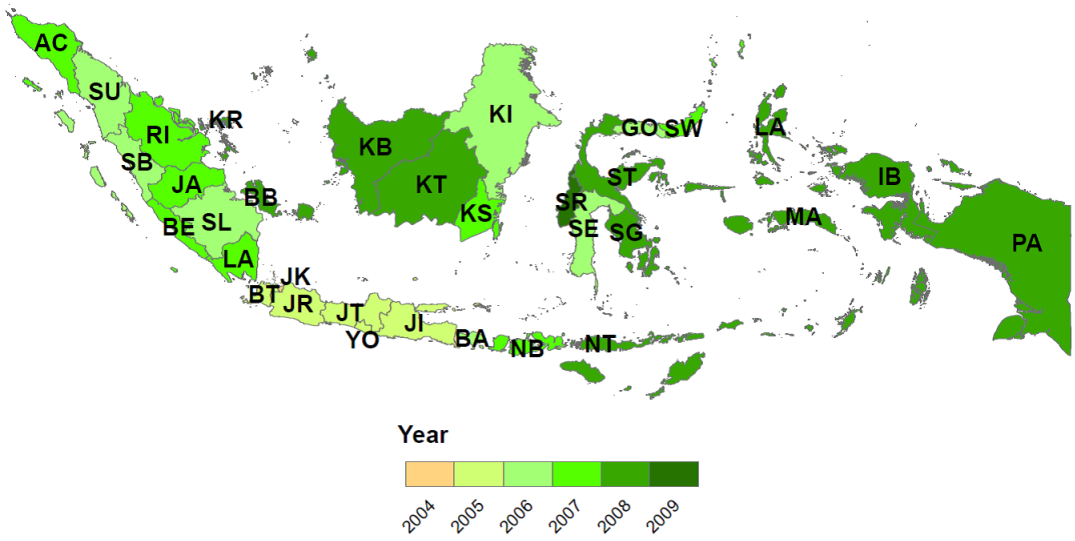


Figure 2.3: *E-procurement adoption - Indonesia*

able online to register expressions of interest, to download detailed bidding and technical qualification documents, to submit pre-qualification materials, and to post questions and complaints. However, due to a regulatory constraint, the final submission of bids was still required to be conducted manually (hence the term "semi") throughout the period we study. The SEP process thus covers the entire procurement process except the final submission of bids. There was no change in procurement rules associated with the switch to SEP; procurement still followed the Presidential guidelines for the procurement of government goods and services issued in 2003 (Government of Indonesia, 2003).[4]

Prior to the adoption of SEP in a province, procurement was in a "copy to internet" (CTI) phase where the acquisition of bidding and technical qualification documents and the submission of bids were carried out manually, but the contract details (bids of each firm and the winner) were posted to the internet ex-post to be publicly available. Crucially for our analysis, the data made public during the copy-to-internet and semi e-procurement systems are identical and in fact use the same electronic platform; the only difference is that procurement actually takes place electronically on the SEP system, whereas in copy-to-

[4]Full e-procurement was not introduced until after the period under study.

internet, information is just released publicly ex-post. We discuss the data in more detail below.

2.2.3 E-procurement: Channels of Influence

There are several mechanisms through with e-procurement could affect outcomes. First, e-procurement could change the composition of bidders. By placing bidding documents online and allowing interactions online rather than in person, e-procurement facilitates bidding at a distance; on the other hand, since it requires internet access, it may be more difficult for unsophisticated bidders. Second, e-procurement may increase the enforcement of government rules and regulations. In the manual procurement system, the paper trail and threat of audit ensures that rules are complied with, but this is imperfect; e-procurement adds an additional layer of intermediation, where the computer system itself enforces certain procedures. Third, e-procurement may prevent denial of access to non-favored suppliers. For example, in a manual procurement system, a procurement official could, for a supplier outside of a pre-specified cartel, claim that there were no copies available of bidding documents or refuse to accept a bid. The e-procurement system cannot be manipulated in this way and provides access to all equally. Finally, even if outcomes are not changed, if e-procurement costs less for the government to administer than a manual procurement process, it could still be beneficial to the government (Singer et al. 2009).

The impact of these changes on prices and quantities is ambiguous for several reasons. First, while most of these effects of e-procurement are "positive" (in the sense of increasing competition and/or reducing the possibility of corruption or collusion), there is one potential downside, namely that it may make access more difficult for bidders with low internet availability or low levels of computer skills.

Second, even conditional on "improving" outcomes for the government, it is not ex-ante obvious whether this improvement will come in the form of lower prices, improved quality, or both (Asker and Cantillon 2010). If e-procurement facilitates the entry of high quality firms from farther away (say, high quality firms located in the capital city), one might expect

quality to rise; prices might even rise as well. On the other hand, if e-procurement facilitates the entry of more firms of similar quality, the impact could either be a fall in prices (if existing firms bid the same quality as before but reduce prices in light of more competition) or an increase in quality (if existing firms increase quality of their proposals for the same price). Either way, these ambiguities suggest that it is important to measure the impacts on both prices and quality when considering the net impact for the government.

These arguments hold true under a competitive model with no collusion or corruption, but similar arguments can be made in cases of corruption, as well. For example, in a corrupt world where the most efficient contractor can pay the highest bribe (as in Tran 2008 and Asker 2010), e-procurement, even if it eliminated the bribes, would not necessarily change the identity of the firm winning the contract (Burguet and Che 2004). It would, however, potentially reduce the price. On the other hand, if there was heterogeneity in firms' willingness to pay bribes, so the most efficient firm was not necessarily the one with the highest willingness to pay, eliminating corruption might also result in a different firm winning the contract and potentially higher quality (Celentani and Ganuza 2002).

Of course, all of these effects could potentially be muted if the requests for proposal changed in response to e-procurement. For example, one might imagine that in response to an intervention that made it more difficult for a procurement officer to direct a contract to a particular bidder, the officer might change the contract requirements such that only the desired firm could satisfy them. While this issue could arise, at least in India this is unlikely to be a major force since the Indian road specifications are largely fixed by PMGSY program guidelines.

2.3 Data and empirical strategy

In this section we describe the Indian and Indonesian datasets, followed by our empirical strategy.

2.3.1 Data

India

We obtain administrative data on costs, quality, and delays associated with each PMGSY contract issued between January 2000 and August 2009 from multiple PMGSY websites. The dataset covers 27 Indian states. For PMGSY, some road packages (sets of construction projects and funding allocated by the federal government) are split into multiple tenders, each with its own contract covering a subset of roads in the package. As a result, our final dataset covers (at least partial information for) 30,578 packages and 35,610 associated contracts.

A first set of outcomes are the cost outcomes associated with each contract. The estimated cost is the amount budgeted (or "sanctioned" in the terminology of PMGSY) by the national authority for construction of a specific road or set of roads in a package. A contract or payment above this amount can only occur with permission from the national offices. The contract value is the amount the government agrees to pay the winning firm for the relevant work. The final payment records the total amount paid out for each package, which as discussed above, may be distributed among multiple contracts and includes any amendments to the contract that occur during construction.

We have measures of two key aspects of project quality: timeliness and physical project quality. For timeliness, we track the time to execution. We use "late completion," which is an indicator variable taking value one if road construction work is not completed by the date agreed to in the initial contract. We also construct a continuous "time overrun ratio" variable, which is the ratio of actual to agreed upon time to completion.

To measure the quality of project implementation, we use quality reports submitted by the National Quality Monitors (NQMs). During construction, PMGSY has a multi-tier quality monitoring system, with local, state, and national monitoring. National monitoring is conducted by NQMs who are retired engineers from other states. The assignment of packages to NQMs is randomized and is therefore conducted similarly in places with and

without electronic procurement.[5] We focus on the outcomes of this national monitoring as our quality outcome. NQMs evaluate each part of the work for material and workmanship as per the format of the standardized "NQM Inspection Report," indicating the tests carried out and the test results obtained. Overall grading includes management issues, contract management, and quality of work. We use a binary assessment (satisfactory/unsatisfactory) of the technical fitness of the road at the time of inspection.

For a subset of packages in the states of Andhra Pradesh, Chhattisgarh, Karnataka, and Uttar Pradesh, we have bidding data. This includes the total number of bids submitted in each auction. Before submitted bids are unsealed, engineers in each state decide which firms have the technical ability (in training, equipment, and experience) to complete the project based on materials firms submit with their bid. We also observe how many of the submitted bids in each auction are judged to be from technically qualified firms.

Indonesia

We scraped data from the CTI and SEP websites of the Indonesian Ministry of Public Works, capturing the complete universe of procurement from 2004 through 2008. Over 14,000 contracts from the MPW national headquarters and the 33 provincial offices are covered. Each entry in the data is at the contract level and specifies the estimated cost, which is, as in India, the maximum amount allowed by the MPW to be paid for a given contract. Information on the type of project is available, as are all bidder names, bid amounts, disqualifications, and final contract values.

We also have the dates on which the notice and details for each contract were first posted online (and concurrently in traditional media), as well as the dates of bid opening and contract award. For road projects (which represent a subset of all projects covered by the Ministry of Public Works), we have a separate database from the road division that tracks

[5]The NQMs are given the letter of request once every two months for carrying out inspection for the forthcoming two months. The NQMs are required to inspect three districts in a single visit in one state in each of the two months. The letter of request allocates a mix of works in progress and completed works. The letter indicates the specific location of works to be inspected and which are in-progress or completed works. Within a block (the administrative unit below district), projects to be inspected are chosen on a random basis.

the start dates and expected and actual completion dates for the services associated with each contract. As with India, we construct "late completion" and "time overrun" variables. No direct quality measure is available for Indonesia. For regressions which consider as an outcome variable the above measures of the timeliness of completion, the number of firms expressing interest (i.e. registering to access bidding documents), or the number of firms bidding, we trim the top and bottom 1 percent of sample observations.

2.3.2 Descriptive Statistics

In the first column of Table 2.1, we present a set of basic descriptive statistics for the Indian data, and in the remaining columns we present statistics from Indonesia. All monetary variables are in logs, which is the form in which they will be used in the regressions below.

Examining the Indian data, in an average year for the typical state, the government sanctioned almost 145 packages worth 9.61 log lakh rupees (approximately US$33 million at exchange rates for 2005, the midpoint of our dataset; 1 lakh is 100,000 rupees) covering over 6.74 log kilometers (525 miles) of road. At the package level, the average log estimated cost of completion over the period observed is 5.211 log lakh rupees (about US$416,000). In the subset of auctions for which we have bidding data (as discussed above, these are from 4 states), there are an average of 2.7 total bids and 1.7 qualified bids.

One contract per package is the mode, but a long right tail is observed. On average, a package is associated with 1.25 contracts for which we have data. Roads contracts generally come in below estimated cost (most likely because exceeding estimated cost requires an additional bureaucratic process). However, cost overruns with respect to the contracted price are relatively common post-contracting. The average payment on completed projects exceeds the average contract value by about 46 percent.

Turning to delays and quality, completion before the contracted due date was rare, with more than three quarters of contracts finished after the assigned deadline and an average delay of 244 days. There was heterogeneity in quality, with 72 percent of projects receiving a satisfactory quality rating on first inspection.

Table 2.1: *Summary statistics*

	India		Indonesia					
	All		All		Works		Consulting	
	Mean (SD)	Obs.	Mean (SD)	Obs.	Mean (SD)	Obs.	Mean (SD)	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State/Province-year level								
Total projects	144.8	158	88.3	166	57.9	164	32.2	160
	(184.9)		(89.1)		(40.3)		(61.2)	
Log total budget	9.610	158	25.674	166	25.610	164	22.886	160
	(1.469)		(0.944)		(0.918)		(1.156)	
Package level								
Log estimated cost	5.211	22,378	20.540	14,657	20.952	9,502	19.780	5,155
	(0.846)		(1.311)		(1.350)		(0.799)	
Contracts in package	1.251	22,378						
	(1.088)							
Log total value of	5.217	22,378						
contracts in package	(0.908)							
Satisfactory quality	0.715	11,200						
at first inspection	(0.451)							
Contract level								
Log contract value	4.529	26,659	20.433	14,623	20.826	9,491	19.706	5,132
	(2.044)		(1.322)		(1.381)		(0.799)	
Log final payment	4.914	14,813						
	(0.903)							
Days completion	244.3	13,781						
delay	(345.4)							
Number of firms			28.837	14,409	33.468	9,329	20.333	5,080
expressing interest			(32.902)		(38.967)		(13.058)	
Number of firms	2.756	1,628	5.842	14,521	7.298	9,414	3.158	5,107
bidding	(3.075)		(4.617)		(5.083)		(1.364)	
Time notice			99.544	10,423	84.393	6,810	128.102	3,613
to award			(110.258)		(100.478)		(121.642)	
Time bid open			29.616	9,199	27.476	5,975	33.583	3,224
to award			(29.672)		(24.182)		(37.477)	
Winner won	0.109	34,126	0.282	9,575	0.212	6,893	0.463	2,682
in first year	(0.312)		(0.450)		(0.408)		(0.499)	
Winner from same	0.302	12,913	0.734	6,096	0.817	4,152	0.556	1,944
district/province	(0.459)		(0.442)		(0.387)		(0.497)	
Time overrun ratio	2.262	13,628	1.876	4,161	2.179	2,986	1.083	1,175
	(3.018)		(0.992)		(0.991)		(0.330)	
Late	0.766	13,628	0.832	4,161	0.948	2,986	0.537	1,175
	(0.423)		(0.374)		(0.223)		(0.499)	

Examining the Indonesian data in Table 2.1, between 2004 and 2008 the average Indonesian province auctioned about 88 contracts per year, with nearly twice as many works as consulting projects, for a province-level average log total budget of 25.67 log rupiah (approximately US$15 million at 2006 exchange rates). At the project level, the average log estimated cost for works projects is 20.95 log rupiah (about US$140,000) and for consulting projects is 19.78 log rupiah (approximately US$43,000). The final contract value for a given project is on average 10 percent lower than the official estimated cost provided by the government prior to bidding. Firms winning contracts are typically based in the same province as the contract, more than 80 percent so for works projects. As with the data from India, delays are common; nearly all works projects (95 percent) and more than half of consulting projects (54 percent) are completed later than the initially agreed upon date.

2.3.3 Empirical Strategy

We estimate impacts separately for India and Indonesia. In both cases, we make use of a difference-in-differences strategy, but our implementation differs slightly across countries, as described below.

In India, for road package or contract i in state s which began in year t we estimate the following OLS specification:

$$y_{ist} = \alpha_s + \beta_t + \mu EPROC_{st} + \mathbf{X}'_{\mathbf{ist}}\gamma + \epsilon_{ist} \tag{2.1}$$

where i is a package or contract, s is a state, and t is a year, and $EPROC_{st}$ is a dummy for e-procurement adoption by state s as of year t. Controls X_{ist} are log estimated cost and log road length.[6] All regressions include state (α_s) and year (β_t) fixed effects. In some cases we collapse the package-level data to the state level and estimate regressions using annual state-level data. In all cases we cluster standard errors by state.

[6]For India, the R-squared values from regressions of e-procurement on log road length and log estimated cost range from 0.0004 to 0.081, depending on the year. For Indonesia, the R-squared values from regressions of project-level e-procurement on log estimated cost range between 0.001 to 0.022 for works projects and between 0.000 to 0.125 for consulting projects, depending on the year.

In Indonesia, the availability of e-procurement at the province level does not necessarily imply that all projects within a province will be contracted using the system.[7] To obtain the average impacts of the use of e-procurement, we instrument for actual e-procurement use with the adoption of e-procurement in the province, as follows. The regression of interest (i.e. the second stage) is:

$$y_{ist} = \alpha_s + \beta_t + \theta EPROCPACKAGE_{ist} + \delta X_{ist} + \epsilon_{ist} \tag{2.2}$$

where $EPROCPACKAGE_{ist}$ is a dummy for e-procurement use in project i in province s as of year t. The project level e-procurement variable $EPROCPACKAGE_{ist}$ is instrumented with $EPROC_{st}$, which is a dummy for whether e-procurement has been adopted by province s as of time t.[8] The control variable X_{ist} is the log estimated cost (since we have many different types of projects and there is no metric available in the data other than estimated cost, we cannot control for road length as for India). Standard errors are clustered by province.

In our regression tables we typically report results for the India sample as Panel A, Indonesia works projects as Panel B and Indonesia consulting projects as Panel C .

2.3.4 Identification Check

Since the adoption of e-procurement is not randomly assigned, we need to ensure that the timing of e-procurement adoption is not correlated with differential trends in procurement that would have occurred in the absence of e-procurement.

To examine this, we begin by checking whether, overall, the adoption of e-procurement is associated with a change in the volume (total budget for procurement) or structure (i.e., across how many contracts procurement is distributed) in a given state or province. Column (1) of Table 2.2 reports regressions where the dependent variable is log total estimated cost

[7]It is possible that this is true for India as well, but for India we do not observe what fraction of packages actually used e-procurement in a given year.

[8]The first-stage coefficient and F-statistic for works projects are, respectively, 0.359 and 40.79 and for consultancy projects are 0.491 and 78.11.

at the state-year level and the explanatory variable of interest is an indicator for any contract being awarded under e-procurement, with log of road length included as a control for India. Column (2) repeats the exercise using number of projects as the dependent variable, controlling for log total state-year estimated project cost. In neither country do we see a significant impact of e-procurement adoption on the total budget allocations (column 1) or the number of projects they are broken into (column 2) at the state/province-level, suggesting that e-procurement was not coincident with major changes in the amount or structure of contracts being procured.[9]

Second, we examine whether the year of official adoption of e-procurement at the state level (conditional on e-procurement occurring after the first two years in our data – that is, after 2005 in Indonesia or after 2001 in India) is significantly related to the difference in log average contract value between the first two years of the data (i.e., between 2001 and 2000 for India (Appendix Table B.1, Panel A) and between 2005 and 2004 for Indonesia (Appendix Table B.1, Panels B and C)). Regressing these early contract value differences on e-procurement can suggest whether differential initial trends predict the year of subsequent e-procurement adoption. We see no correlation between change in state-level contract value and the timing of e-procurement for either India or Indonesia.

2.4 Results

2.4.1 Did E-procurement Change the Contracting Process?

Contracting Execution

In Table 2.3 we first examine whether the introduction of e-procurement has an impact on the duration of the project contracting phase. In columns (1) and (2) we consider the time elapsed between tender notice and contract award and between bid opening and contract award. Both variables are available only for the Indonesia sample. While neither variable is significantly impacted for works projects, we find an increase of 54.15 days (statistically

[9]We find a similar null result for e-procurement's effect on log road length.

Table 2.2: *Budget impact*

	State-year level	
	Log total estimated cost at state-year (1)	Number of projects in state-year (2)
Panel A. India		
E-procurement	-0.041	-54.39
	(0.089)	(60.72)
Mean Dep. Var. (Non-Eproc)	9.55	139.20
	(1.41)	(184.07)
Observations	157	157
Panel B. Indonesia - Works Projects		
E-procurement (IV)	0.187	-3.559
	(0.151)	(7.069)
Mean Dep. Var. (Non-Eproc)	25.211	45.102
	(0.774)	(28.803)
Observations	164	164
Panel C. Indonesia - Consultancy Projects		
E-procurement (IV)	0.374	7.641
	(0.226)	(8.486)
Mean Dep. Var (Non-Eproc)	22.306	15.831
	(0.888)	(10.009)
Observations	160	160

Notes: India - Column (1) reports estimates from an OLS regression of log total estimated cost at the state-year level on an indicator for any contract being awarded under e-procurement in the respective state and year. Column (2) does the same using number of packages as the dependent variable. Log of road length and log of estimated cost are included as controls when not on the LHS. Indonesia - Columns (1) and (2) give results from OLS regressions at the state-year level, where the dependent variable is given in the table and the independent variable is the official adoption of electronic procurement at the state level. Column (2) includes a control for log total state-year estimated project cost. Both regressions include state and year fixed effects. Standard errors clustered at the state level are in parentheses below estimates. Means for each dependent variable are also reported, with standard deviations in parentheses. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

significant at the 1 percent level), a more than 50 percent increase, in the time elapsed between the tender notice and awarding of contract for consulting projects. In conjunction with the absence of an effect in column (2) on the amount of time between bid opening and award, this implies that for consulting projects e-procurement increased the period of time between the tender notice and the opening of bidding, in which firms can learn about potential contracts and prepare their bid documents.

Who Bids and Who Wins?

We next consider indicators of changing contractor identity. In column (3) of Table 2.3 we investigate the impact of e-procurement on the number of firms who express an interest in bidding. These data are only available for Indonesia. For nearly every works project, this process entails registering to be allowed to download the detailed bid documents and participate in bid submission. For all consulting projects and a small subset of works projects, expression of interest involves the submission of a pre-qualification document on which firms are scored and a subset of those passing are chosen to submit bids. The number of firms expressing interest in the case of works projects more than doubles.

In column (4) we examine the number of firms submitting a complete bid. This number is relatively low in India with the average manual procurement contract receiving roughly 2.9 bids. This number is higher for Indonesia at over 7 bids per works project and 3.2 per consulting project. However, in no instance does the average number of bids increase due to e-procurement.

Column (5) examines whether e-procurement changes the likelihood that the winning firm is based in the same area in which a project is tendered. While a variable for contractor home province was available directly in the Indonesian data, we proxy for firm default location in India with the modal district among those in which the contractor was observed with contracts prior to e-procurement starting anywhere in the country.[10] In general, as

[10]In India, the only consistent firm identification data we observe is a unique ID within states which does not link across states. Since some firms are not observed prior to the start of e-procurement, the location variable is undefined for these firms and the associated observations are excluded from this regression.

Table 2.3: *Contract process*

	Time elapsed notice to award (1)	Time elapsed bid open to award (2)	Number of firms expressing interest (3)	Number of firms bidding (4)	Winner from same province/ district (5)	Winner in first year (6)
Panel A. India						
E-procurement				0.364	-0.108***	0.0222
				[0.733]	(0.029)	(0.0218)
Mean Dep. Var. (Non-Eproc)				2.86	0.581	0.141
				(3.52)	(0.493)	(0.348)
Observations				1,406	6,545	26,246
Panel B. Indonesia - Works Projects						
E-procurement (IV)	48.75	-2.54	24.735*	0.420	-0.036	0.069
	(38.24)	(6.93)	(13.657)	(1.520)	(0.057)	(0.060)
Mean Dep. Var. (Non-Eproc)	64.90	25.33	17.456	7.005	0.816	0.245
	(71.20)	(29.43)	(21.460)	(5.057)	(0.387)	(0.430)
Observations	6,804	5,974	9,323	9,408	4,151	6,892
Panel C. Indonesia - Consultancy Projects						
E-procurement (IV)	54.15***	9.88	-3.489	-0.242	-0.233*	0.414**
	(19.10)	(6.19)	(2.561)	(0.281)	(0.133)	(0.183)
Mean Dep. Var. (Non-Eproc)	98.01	26.13	11.069	3.178	0.650	0.446
	(101.20)	(48.53)	(6.174)	(1.468)	(0.477)	(0.497)
Observations	3,611	3,223	5,078	5,107	1,944	2,682

Notes: India - All columns report OLS estimates from a regression at the contract level of the listed variable on an indicator for the contract being awarded using e-procurement. Column (4) is estimated on a subset of contracts for which we have bidding data in the states of Andhra Pradesh, Chhattisgarh, Karnataka, and Uttar Pradesh. Column (5) defines the district which a contractor is from as the modal district of the contractor's observed contracts prior to e-procurement starting anywhere in the country. This regression is therefore restricted to contracts won by the cohort of contractors who were awarded contracts prior to e-procurement. Winner in first year (Column (6)) is an indicator for the winner having been awarded a contract in 2000 or 2001. This regression is restricted to observations after the first year. State and year fixed effects as well as controls for log of road length and log of estimated cost are included. Indonesia - All columns report IV estimates at the project level, where the dependent variable is given in the table and the independent variable is the contract-level use of electronic procurement, instrumented by the official adoption of electronic procurement at the state level. Values of time elapsed in columns (1) and (2) were available for a subset of provinces in 2004 and all provinces in subsequent years. Column (5) defines the province where the contractor is from as the province as that directly indicated in the data. This regression therefore excludes contracts based out of the national headquarters. Column (6) defines winner in first year as an indicator for the winner having been awarded a contract in 2004. This regression is restricted to observations after the first year in the data. State and year fixed effects as well as a control for log of estimated cost are included. Standard errors are clustered at the state level. Where brackets are used, p-values are given using the wild bootstrap method. Non-e-procurement project means for each dependent variable are also reported, with standard deviations in parentheses. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

with most construction projects, where moving equipment is quite costly and where there is often a need for local sources of materials (e.g., hot asphalt), there is a strong tendency to be local: In India nearly 60 percent of winning bids come from firms located in the same district and in Indonesia over 80 percent of works contracts are won by firms from the same province.

We observe a significant 11 percentage point decline in this variable in India and a 23 percentage point decline for consultancy projects in Indonesia. Given that it is much easier to move engineers geographically than to move heavy equipment and asphalt plants, it is not surprising that the impacts for Indonesia – where cross-provincial distances are much greater than the cross-district distances we observe in India – are concentrated among consulting projects.

Finally, we examine in column (6) whether e-procurement changed entry of new firms. Specifically, we examine whether the winning bidder was present and won a contract bid in the first year of our data (2000 or 2001 for districts in India or 2004 for provinces in Indonesia). This regression is restricted to observations which fall in years subsequent to these initial years. We observe a highly significant increase of 41 percentage points for consultancy projects in Indonesia, showing that e-procurement leads to more contracts being won by pre-existing winning firms. Combined with column (5), this suggests that e-procurement leads to projects being won by pre-existing firms from other provinces.

One consistent explanation for these findings is that e-procurement improves access to information for firms outside the area where procurement is taking place. This is a relatively larger benefit for firms that are farther away and may also favor pre-existing firms with a documented record of success.

2.4.2 Did E-Procurement Change Procurement Outcomes?

There are three main outcome variables about which the government cares when procuring for the provision of a service/construction of a project: the price it pays for the contract, the timeliness with which the contract is executed, and the quality of work undertaken. In this

section we examine the impact of e-procurement on each of these three dimensions.

Prices

To examine the impact on prices paid, we first consider impacts in each country on the contract value agreed upon between the winning bidder and the government. Column (1) of Table 2.4 shows no statistically significant impact in India on log contract value, conditional on the log length of the road and the log estimated cost of the road. The point estimate is quite small – 0.02 – and the 95 percent confidence interval ranges from -0.027 to 0.064. This means that we can reject the hypothesis that there was more than 3 percent cost savings associated with e-procurement, based on the original contract value. For Indonesia, conditional on the log estimated cost, the point estimates for both works and consultancy projects suggest small reductions in log contract value, though they are not statistically significant. The confidence intervals suggest that, at 95 percent confidence, we can rule out price declines in Indonesia of more than 14.1 percent for works projects and of more than 6.6 percent for consulting projects.

Of course, the government does not care about the contract value, per se; rather, the more important measure is the actual amount paid to the contractor. Cost overruns are frequent, so as discussed above, the amount paid is typically higher than the contract value. Column (2) examines the impact on the final amount actually paid by the government, including any overruns or contract amendments. Values for this variable are only available for India. Note that the observation count falls substantially here since not all packages were complete at the time of data collection.[11] Again, no significant impact is observed. The point estimate for India is a 2.5 percent drop in final payments, with the 95 percent confidence interval ranging from a 8.7 percent decrease to a 3.7 percent increase. Given the uncertainly around this point estimate and that of the effect on contract values, we are not able to infer whether changes in final payments, if any, came through shifts in contract

[11] As we show subsequently, the date to completion is unaffected by e-procurement, so this type of sample selection is unlikely to be of concern.

Table 2.4: *Prices and project execution*

	Prices		Delays		Quality			
	Log contract value (1)	Log final payment (2)	Late complete (3)	Time overrun ratio (4)	First quality grade (5)	First quality grade (cond. on complete) (6)	Minimum across quality grades (7)	Minimum across quality grades (cond. on complete) (8)
Panel A. India								
E-procurement	0.0184	-0.0249	0.00116	0.127	0.0113	0.123**	0.0442	0.194***
	(0.0233)	(0.0316)	(0.0668)	(0.233)	(0.0277)	(0.045)	(0.0448)	(0.033)
Mean Dep. Var. (Non-Eproc)	5.198	4.959	0.779	2.337	0.710	0.827	0.584	0.778
	(0.843)	(0.794)	(0.415)	(3.117)	(0.454)	(0.378)	(0.493)	(0.416)
Observations	21,980	11,237	13,462	13,462	11,350	1,783	11,350	1,783
Panel B. Indonesia - Works Projects								
E-procurement (IV)	-0.049		-0.161*	0.122				
	(0.047)		(0.089)	(0.234)				
Mean Dep. Var. (Non-Eproc)	20.766		0.956	2.177				
	(1.329)		(0.204)	(0.976)				
Observations	9,485		2,985	2,985				
Panel C. Indonesia - Consultancy Projects								
E-procurement (IV)	-0.023		0.348	0.143				
	(0.022)		(0.231)	(0.238)				
Mean Dep. Var. (Non-Eproc)	19.502		0.610	1.110				
	(0.841)		(0.488)	(0.429)				
Observations	5,130		1,175	1,175				

Notes: India - Columns (1), (2), and (5)-(8) report OLS estimates at the package-level of the listed variable on an indicator for any contract in the package being awarded under e-procurement. Columns (3) and (4) report contract-level estimates of the same. Column (5) includes controls for complete at first inspection, first inspection year, and inspector. Column (6) includes controls for first inspection year and inspector. State and year fixed effects and controls for log road length and log estimated cost are included in all regressions. Indonesia - All columns report IV estimates at the project level, where the dependent variable is given in the table and the independent variable is the contract-level e-procurement use, instrumented by state-level official adoption of e-procurement. All regressions include state and year fixed effects and a control for log estimated project cost. The variables in columns (3) and (4) were constructed using data available for a subset of projects from the roads division. Standard errors are clustered at the state level. Non-e-procurement project means for each dependent variable are also reported, with standard deviations in parentheses. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

values or changes in cost overruns. On net, the evidence in this section suggests that there was no statistically detectable impact on prices paid for projects, and to the extent there are effects we cannot detect, they are not overwhelming in size.

Project Timeliness

We next examine the first measure of quality of execution: delays in the execution of projects. In column (3) of Table 2.4 we consider late completion – a dummy taking value 1 if a project is finished after the contracted completion date and 0 otherwise.[12] Late completion is very common in both countries. More than three quarters of projects in India are completed late, while 95 percent of works projects and more than half of consultancy projects in Indonesia are not finished on time. In the case of Indonesian works projects, we see a significant 16 percentage point decline in this variable associated with the adoption of e-procurement. We then consider the time overrun ratio in column (4). This is the actual time to completion divided by the contracted time to completion. For both India and Indonesian works projects, we observe high levels of overrun – on average, actual time to completion is more than double the contracted time. However, in neither country do we see a significant decline associated with adoption of e-procurement.[13]

Quality

In the final four columns of Table 2.4, we turn to package-level physical quality measures from a nation-wide auditing process. As described above, these data only exist for the India sample. The Indian National Quality Monitoring process is coordinated centrally, and inspectors audit a randomly selected bundle of roads (both in progress and complete). Specifically, for auditing roads in a given season, the national monitor is told how many

[12]We do not find significant effects of e-procurement on target timeline length in either India or Indonesia.

[13]Note that for Indonesian works, the point estimate on the time over-run ratio is positive while the dummy on late completion is negative. This is due to a few extreme outliers. If we trim the bottom and top 2.5 percent of observations, the coefficient and statistical significance in column (3) are essentially unchanged, but the point estimate in column (4) becomes -0.025, not statistically significant.

of each type of project (complete and ongoing) to sample and from which districts in a state. He then separately samples at random from the lists of projects provided by the state road department. In addition, inspectors are allocated follow-up audits for roads, and here, poorly graded roads are oversampled.

Thus, only the first quality grade is randomly selected. We examine two variables: the first quality grade and the minimal quality grade for the road project given by the national monitor. The minimum quality grade is the lowest quality grade given during any of the inspector evaluations across all contracts in the package. The quality score is measured as either 0 (unsatisfactory) or 1 (satisfactory). We separately consider quality outcomes for all projects (including those in progress) and for only completed roads.

In columns (6) and (8), we find highly significant impacts on both the first and minimum quality grades for completed roads. E-procurement is associated with increases of 12.3 and 19.4 percentage points in the first quality grade and worst quality grade, respectively. In contrast, we do not see any significant impacts on incomplete works (columns 5 and 7). The evaluation of incomplete projects may be inherently prone to greater measurement error because a project is behind schedule, being revised, or is simply not far along enough to be accurately judged. We see no evidence in the data that e-procurement leads to more abandoned (never completed) contracts or to a differential number of incomplete projects at the time of inspection, suggesting that the difference in significance of the quality estimates cannot be explained by e-procurement driving low-quality contracts to an even lower level that results in unfinished construction.

2.4.3 Selecting Better Contractors, or Making Existing Contractors Better?

We observe significant impacts of e-procurement on road quality in India and evidence of reductions in delays in Indonesia. We also find that winning contractors are more likely to come from outside areas. We now explore a potential channel of influence: whether e-procurement changed the pool of contractors who bid and caused better contractors to be selected.

To the extent that e-procurement improves outcomes, an important question is whether it does so by selecting better contractors, or by encouraging existing contractors to perform better. Selection effects would occur if the main effects described above were primarily driven by e-procurement spurring new entrants, and if those new entrants won; treatment effects on existing contractors could occur if increased competition or lower corruption led these incumbent firms to submit higher quality bids.

To investigate these possibilities, we conduct a two-stage strategy where we first estimate a fixed effect for each firm in the data. This measures each firm's average quality. We then examine whether e-procurement leads to the selection of higher quality firms. If it does – and if these coefficients match the overall effects documented above – we can then conclude that selection effects play a large role. On the other hand, if e-procurement does not change average firm quality, then the quality and delay effects documented above must be arising through increased performance from a given set of incumbent firms.

For each outcome of interest y, we initially estimate the following regression:

$$y_{icst} = \tau_c + \alpha_s + \alpha_t + \delta EPROC_{st} + \mathbf{X'_{icst}}\gamma + \epsilon_{icst} \tag{2.3}$$

where τ_c is a winning contractor fixed effect and $EPROC_{st}$ is a dummy for e-procurement adoption in state s as of year t.[14] We include state/province and year fixed effects and controls as before. We generate a full set of contractor fixed effects from this regression and then use them as the outcome variable in the following regression:

$$\tau_{ist} = \alpha_s + \alpha_t + \beta EPROC_{st} + \mathbf{X'_{ist}}\gamma + \epsilon_{ist} \tag{2.4}$$

where τ_{ist} is the estimated τ_c from the initial regression for the contractor winning project ist. The estimation is weighted by the inverse variance of τ_c (as estimated from the first stage). This regression asks how e-procurement affected the choice of which contractors

[14]For Indonesia, we additionally include the dummy variable, $EPROCPACKAGE_{icst}$, to account for the fact that e-procurement adoption at the state level does not imply that all projects in that province will be tendered using e-procurement.

won a given package.[15] Standard errors are clustered by both state and winning contractor in each stage.

Examining prices in columns (1) and (2) of Table 2.5, we find no evidence suggesting that the introduction of e-procurement was accompanied in either India or Indonesia with the selection of winning firms that tend to make lower or higher winning bid amounts for a given estimated cost.

We then examine delays. In column (3), we see that for works projects in Indonesia, e-procurement was associated with firms that tended to be less late being more likely to win. The magnitude of the coefficient – 0.058 – is about one-third of the equivalent magnitude in column (3) of Table 2.4 – 0.161 – which suggests that one-third of the effect is driven by changes in selection, while two-thirds of the effect is driven by existing contractors performing better.

We also find substantial impacts on the average quality of contractors, as shown in columns (6) and (8). The magnitudes of these coefficients are between fifty and eighty percent of those for the overall quality effects in Table 2.4. This suggests that the quality effect is driven primarily by higher quality contractors being selected rather than by increased competition improving the quality of existing contractors.[16]

2.5 Conclusion

This paper provides some of the first rigorous evidence on the impact of e-procurement on contractual choice and subsequent contract outcomes. All told, the results present a

[15] For Indonesia as before, we use $EPROCPACKAGE_{ist}$ instrumented with $EPROC_{st}$ in this regression.

[16] In addition to improvements in terms of price and quality, ensuring the participation of smaller firms may be an outcome that is desirable from the perspective of the government and influenced by the structure of procurement (Krasnokutsaya and Seim, 2011). To address the possibility of such effects here, we examine whether winning contractors under e-procurement are more likely to be large firms, as proxied by whether they tend to be involved with larger projects (results available from authors). In India, e-procurement is associated with such a change when we consider the outcome of log road length, but there is no significant impact for estimated project cost. In Indonesia, we see some increase in contractor size for works projects but it is very noisily estimated, while no effect is observed for consulting projects. These results suggest that e-procurement results in minor, if any, changes in the distribution of winning firm size.

Table 2.5: *Two-stage contractor fixed effect regressions*

	Prices		Delays		Quality			
	Log contract value (1)	Log final payment (2)	Late complete (3)	Time overrun ratio (4)	First quality grade (5)	First quality grade (cond. on complete) (6)	Minimum across quality grades (7)	Minimum across quality grades (cond. on complete) (8)
Panel A. India								
E-procurement	0.00206	0.00568	0.0119	0.218	0.0214	0.0973**	0.0181	0.107**
	(0.01211)	(0.01315)	(0.0403)	(0.154)	(0.0201)	(0.0460)	(0.0253)	(0.049)
Observations	18,745	10,161	9,997	9,997	9,346	1,789	9,346	1,789
Panel B. Indonesia - Works Projects								
E-procurement (IV)	0.0642		-0.058**	-0.127				
	(0.0546)		(0.025)	(0.138)				
Observations	8,352		2,941	2,939				
Panel C. Indonesia - Consultancy Projects								
E-procurement (IV)	0.000007		-0.003	0.008				
	(0.00393)		(0.068)	(0.024)				
Observations	4,371		948	955				

Notes: India - The first stage is an OLS estimate, where the dependent variable is given in the table and regressed on a set of winning-contractor fixed effects. The second stage regresses the estimated contract fixed effects for each contract on an indicator for the contract being awarded using e-procurement. Coefficients on the indicator from the second stage are reported. In the second stage, observations are weighted by the inverse of the winning-contractor estimate squared standard errors. All regressions are run at the package level and are restricted to packages with one contract. State and year fixed effects as well as controls for log road length and log estimated cost are included in the second stage, while a control for e-procurement is additionally included in the first stage. Indonesia - Observations are at the project level. The first stage is an OLS estimate, where the dependent variable is given in the table and regressed on a set of winning-contractor fixed effects. The second stage is an IV estimate, where the dependent variable is the winning-contractor coefficient from the first stage and the dependent variable is the use of e-procurement, instrumented by the official adoption of electronic procurement at the state level. In the second stage, observations are weighted by the inverse of the winning-contractor estimate squared standard errors. All regressions include state and year fixed effects in both stages as well as a control for log estimated cost in the second stage, while controls for use of e-procurement at the project level, and adoption of electronic procurement at the state level are additionally included in the first stage. Standard errors are clustered at the state and winning contractor levels. For descriptions of the dependent variables, see Table 2.4. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

consistent story. E-procurement appears to have led to increased ability of firms from outside the home region to win contracts. These firms, in turn, tended to be higher quality firms in general, as measured by their average delay times (in Indonesia) and average construction quality (in India). This led to improvements in the quality of roads and timeliness but no detectable changes in price.

Following the increase in competition among firms for contracts brought about by the adoption of e-procurement, a reduction in the rents accruing to winning firms may be expected to occur either through an increase in quality for a given price or a decrease in price for a given level of quality. In both the Indian and Indonesian settings examined here, we see evidence in support of improvements in quality for a given price. The fact that we observe changes on the quality margin, and that it occurs through changing which contractors win rather than the performance of a given set of winning contracts, suggests that the system prior to e-procurement was not necessarily selecting the most efficient firms, and that e-procurement may have improved efficiency even if it did not necessarily lower prices paid. It also suggests that the practice of giving contracts to the lowest price bidder likely contributed to greater inefficiencies on the quality margin.

Overall, our findings provide qualified support to the view that e-governance can improve the provision of public services. E-procurement was a partial reform which changed the application process but neither the rules for technical qualification nor the requirement that the lowest price bidder receive the contract. Case-study evidence suggests that official discretion in determining technical disqualification is often an important way of limiting competition. Equally, emphasizing price over quality (as occurs when lowest price bidder always wins) implies that bidders build in their profit margins by cutting back on quality. E-procurement facilitated entry but left significant discretion with officials in determining qualification, and the bidding rules continued to prioritize low price over quality. Thus, it appears that the main impact we see can be attributed to gains from trade associated with having reduced barriers to entry. It is an open question whether a reform package which changes both the application process and also the process of selecting among

bidders could lead to even larger gains in economic efficiency.

Chapter 3

Out of Sight, Out of Mind: Impacts of Open Meetings in U.S. State Legislatures

3.1 Introduction

Increases in institutional transparency in democracies are commonly assumed to improve outcomes of policy making. Efforts have been undertaken in the United States to increase the information available on national legislator behavior through the government creation and maintenance of publicly available online databases (Lessig, 2009). The European Parliament has also recently demanded greater transparency from the EU Council of Ministers (Gräßle, 2009). Additionally, as nascent democracies in the developing world refine their institutional structures, increasing openness across the various branches of government is often a central goal. While proponents of greater transparency emphasize the benefits of increased accountability and responsiveness of elected representatives, theory suggests that greater transparency need not lead to beneficial consequences for the general public, even excluding special cases such as the possible jeopardy of national security or violation of individual rights.

This paper provides empirical evidence in the U.S. setting on the effects of the enactment of statutes that guarantee public access to state legislatures, hereafter referred to as open meetings laws. I first examine the impact of open meetings on the total numbers of bills introduced and enacted and on the timeliness with which the state budget, arguably the most important piece of legislative output, is delivered. As a more direct measure of whether legislators are incentivized to take actions with important economic impacts, state government expenditure is considered as an outcome. I finally examine how open meetings influence citizens' perceptions of government and their behavior in elections to state legislatures.

In addition to considering the average impacts of open meetings on these outcomes, I investigate how the impacts of open meetings may be mediated by the level of geographic isolation of the state legislature from its constituents. Campante and Do (2014) find that states with more isolated capital cities, in terms of average log weighted distance from the population, have weaker accountability and worse government performance. It is reasonable then to expect that the effects of increasing transparency through open meetings may differ in the isolation of the state government, due to differences across states both in the baseline level of quality of government and in the effective magnitude of the change in public informedness associated with open meetings.

I find first that open meetings decrease legislative enactments by roughly 11 percent, but do not impact the amount of bills introduced. No effects on average are observed on the timeliness of state budgets. Open meetings increase expenditure on public goods by more than 5 percent on average. I also find evidence that these expenditure effects are concentrated in areas with greater state capital isolation, where government spending on public goods is lower to begin with.

Open meetings also shift citizens' perceptions of state government, such that respondents are 20 percent more likely to express at least moderate confidence in the government. An examination of heterogeneity in impacts by capital isolation shows that the effects are driven by gains in low isolation areas. In states with a greater average log weighted distance of the

population from the capital, open meetings actually increase the proportion of respondents choosing the lowest possible measure of confidence.

Mirroring this pattern, I observe significant differences in the impact of open meetings on voting outcomes by the isolation of the capital. Whereas incumbents in low isolation locations see their vote shares increase with open meetings, those where the capital is more remote experience a decrease in vote share. Given that incumbents in elections to state legislatures win on average by more than 30 percentage points, these vote share effects are unsurprisingly not large enough to significantly impact the likelihood of incumbent victory.

This paper contributes to a literature demonstrating the importance of public awareness in upholding the monitoring and accountability of politicians. Ferraz and Finan (2008) find that re-election impacts on Brazilian mayors following random audits depend both on the severity of corruption problems revealed in their municipalities and the level of radio coverage. Besley and Burgess (2002) show that state governments in India increase public relief in response to negative agricultural shocks when newspaper circulation is higher. Humphreys and Weinstein (2012) provide experimental evidence in the Ugandan context that the dissemination of parliamentary scorecards to the public did not lead to improved politician performance or election impacts, and suggest that this is at least in part because constituents remained poorly informed despite dissemination campaigns.

Djankov et al. (2010) conduct a 175 country study which finds that public disclosure of income sources and business connections of MPs is associated with lower perceived corruption and better government. In contrast, they find no similar systematic relation when disclosure is made only to specific government agencies, indicating that public awareness is important in holding politicians accountable. In the U.S. context, Snyder and Strömberg (2010) determine that House representatives subject to increased press coverage exhibit greater effort in serving their constituencies and capture a larger share of votes when running for re-election. This paper focuses on the impacts of an alternative mechanism, open meetings, through which information on the activity of elected officials is made more accessible to the public.

The paper proceeds as follows: Section 3.2 provides a conceptual framework. Section 3.3 describes the background and data, and Section 3.4 covers the identification strategy. Section 3.5 details the results. Section 3.6 concludes.

3.2 Conceptual framework

Is greater transparency necessarily beneficial?

A sizeable body of work has provided theoretical and, increasingly, empirical evidence in support of the beneficial impacts of heightening the transparency of democratic governments. With improved access to information, the argument goes, citizens are better able to evaluate the performance and quality of politicians and hold them accountable via the electoral process, reducing the scope for moral hazard and over time improving the selection of officials.

It is not a given, however, that greater transparency will necessarily improve outcomes from the public perspective. Prat (2005) presents a model that highlights the importance in a principal-agent setting of distinguishing between revealing more information about agent actions versus about the consequences of those actions. In contexts where it is difficult to evaluate fully the consequences of agent choices, increasing information about the actions themselves may increase the incentive of the agent to disregard valuable private information when choosing what to do ("conformism").

Relatedly, Stasavage (2004) considers settings where decisions require compromise among multiple agents. He demonstrates that under certain circumstances increasing the transparency of the bargaining process can lead to costly delays or breakdown in the decision making process, as representatives' incentive to adopt less compromising bargaining positions in an effort to signal quality to constituents is strengthened ("posturing"). As bargaining among multiple representative is inherent to the legislative process and open meetings laws primarily increase the information available to the public about the actions of legislators, as opposed to their consequences, concerns of these types may be particularly

relevant.

How do impacts vary with state isolation?

Campante and Do (2014) establish that the isolation of state government from the public is robustly associated with greater corruption, worse public goods provision, and weaker accountability – lower newspaper coverage of state politics, reduced voter knowledge of and interest in state politics, and lower turnout in state elections. State capital isolation may then mediate the impacts of the adoption of open meetings in legislatures through two channels. First, for a given increase in transparency, the content of the new information being revealed may differ. In more isolated states, where the status quo is one of lower quality government performance, increasing the information available to the public may give re-election-minded officials a stronger incentive to take action in response (though given the monitoring/accountability vs conformism/posturing tensions, whether this is ultimately beneficial from the public perspective is ambiguous).

Second, a given de jure change in transparency may differ in its de facto impact on information levels depending on the initial isolation of the legislature. In less isolated locations, the public may already be relatively well-informed, so that adoption of open meetings results in a smaller effective increase in information. This impact would be in the same direction as the content effect, with legislators reacting more strongly to open meetings in more isolated settings. On the other hand, uptake of newly available information may be lower in more isolated locations due to weaker newspaper coverage of politics and lower voter interest. Additionally, if more isolated state capitals are more corrupt, legislators may be more adept at circumventing open meetings regulations or otherwise suppressing information. These latter two possibilities suggest that in more isolated locations open meetings would yield a smaller effective increase in the publics' knowledge, dampening legislators incentives to change behavior in response.

Overall, it is theoretically ambiguous whether on average the adoption of open meetings in the legislative setting is beneficial from the public perspective and how this may vary

across states with more or less isolated capitals. Given the theoretical uncertainty, an empirical investigation is valuable in helping to improve our understanding of which of the potential forces are dominant in the U.S. legislative setting and whether future initiatives aimed at further increasing state government transparency should be tailored differently depending on the isolation of the state capital.

3.3 Background and data

3.3.1 Open meetings

The primary source of information on state level open meetings laws used in this paper is The Open Government Guide, published by The Reporters Committee for Freedom of the Press. The aim of this guide is to provide comprehensive information on open government law and practice in each of the fifty states and the District of Columbia. It provides outlines detailing the history of open government laws in each state and the rights of reporters and citizens to attend meetings and obtain records of the various branches of state and local governments.[1] The guide also gives information on compliance and enforcement of the laws in each state, as well as relevant related court decisions. These outlines were complemented by information gathered from various Attorney General's opinions, the Book of the States, and Ballotpedia.org.

Using these sources, I determine for each year whether a statute requiring open access to government meetings that applies to the state legislature had yet been enacted,[2] taking into account information on how binding are the requirements of each statute, i.e. whether or not unclear language or other potential loopholes were included.[3] As shown in Figure

[1]Volunteer attorneys with expertise in open government and access laws in each state prepared the outlines in 1989 and have provided updates regularly since, most recently in 2011.

[2]While some states may have open meetings precedents set in common law or constitutional provisions which can be interpreted as requiring them, I focus on statutory law because it in general defines more clearly the degree to which open meetings laws are applicable to the state legislature and is therefore more binding.

[3]I also consider whether later legislation or judicial rulings weakened the original law, so that a state may shift back to not having open meetings. This occurs in 3 instances: Alaska (1987), Indiana (1993), and Wisconsin

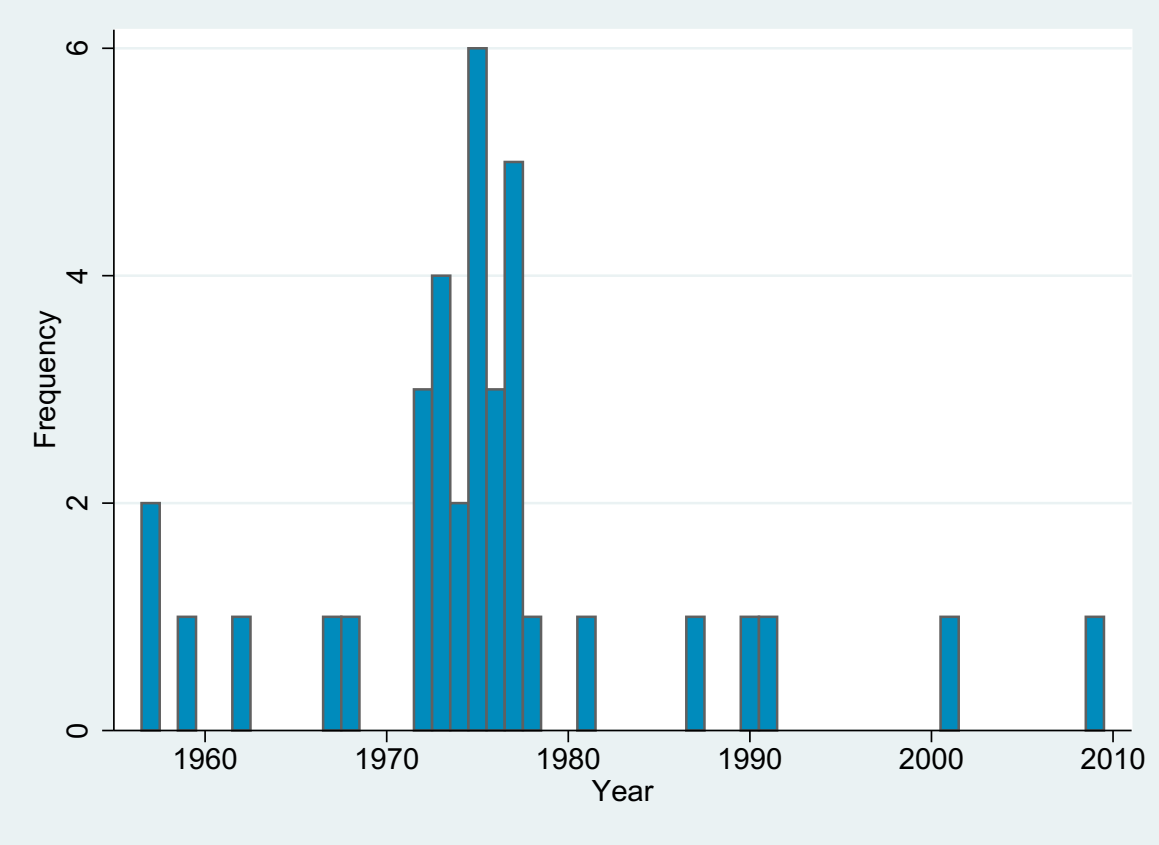


Figure 3.1: *Timing of open meetings adoption across states*

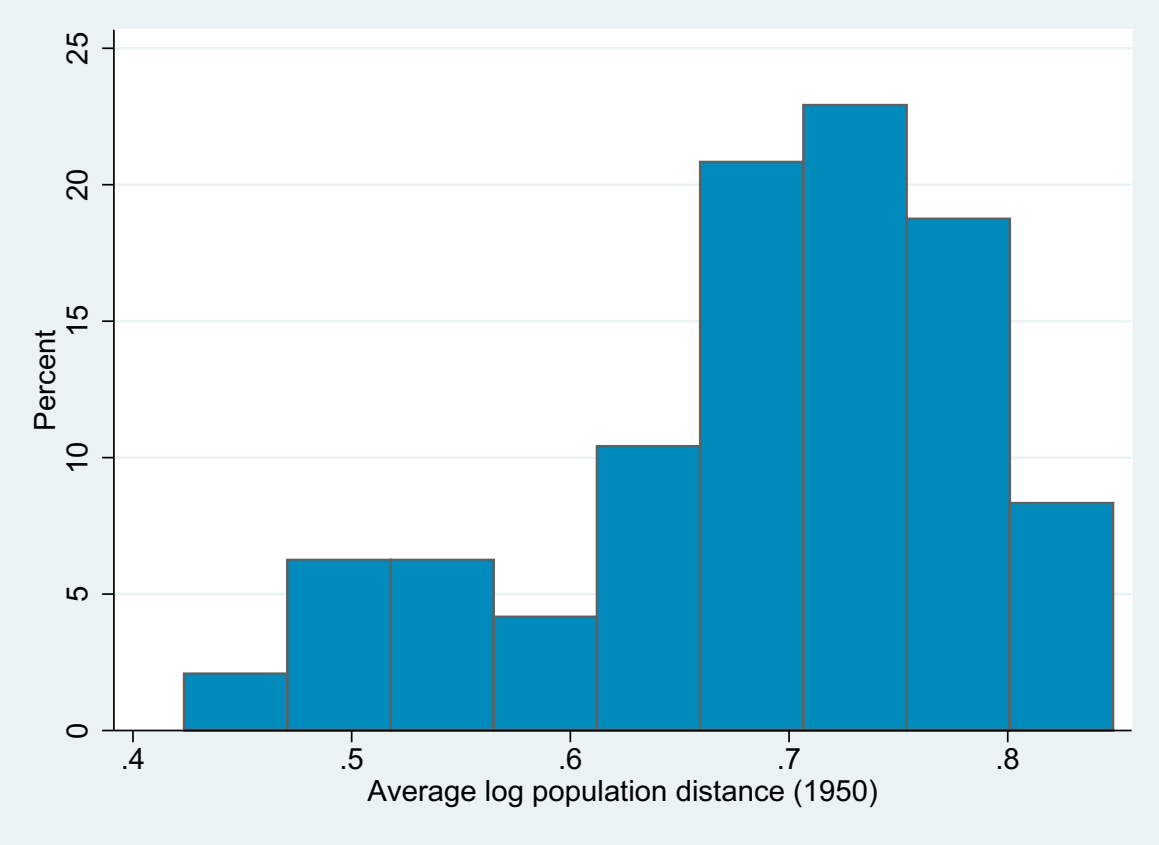
3.1, 36 states switched to having such an open meetings law between 1950 and 2010, with 75 percent of these changes occurring betwen 1972 and 1978.

3.3.2 State capital isolation

I use the measure of state government isolation from Campante and Do (2014), the average log distance of the state's population to the capital city.[4] The authors find that isolated state capitals are strongly associated with greater corruption, lower public goods provision, and weaker accountability – lower newspaper coverage, lower voter interest in and knowledge of state politics, and reduced turnout in state elections. The authors compute the measure

(2011).

[4]More precisely, the log county population weighted average distance of each county centroid from the state capital, defined as the coordinates of the State House or Assembly.



Notes: Figure presents the distribution across states of the 1950 value of the state capital isolation measure from Campante and Do (2014) - log county population weighted average distance of each country centroid from the state capital, defined as the coordinates of the State House or Assembly. Not available for Alaska or Hawaii.

Figure 3.2: *Distribution of state capital isolation*

for each of the 48 continental states in each census year between 1920 and 2000. To remain consistent in the government isolation variable definition, given that I consider outcomes from as early as 1951, I use states' log average distance values from 1950 in all regressions.[5] The variable ranges in value between least isolated at 0.423 to most isolated at 0.848, with a mean of 0.688 (see Figure 3.2).

[5]Results are little changed throughout with use instead of the 1920 values, 1920-1950 means, or, for outcomes in later time ranges, the 1960 or 1970 values.

3.3.3 Legislative measures

The primary measures considered by legislatures are bills and resolutions. A bill is a proposal to establish a new law or to change or repeal an existing law, while resolutions vary in force of law and often merely express sentiments or opinions of the legislature. I therefore focus on bills in my analysis. The typical process of bill consideration begins with the drafting and introduction of a bill to a house of the legislature by one of its members. The bill is then referred to a committee based on subject matter. If the committee reports in favor of the bill, the measure is then considered by the entire house. Contingent upon a majority vote to pass the bill, it is referred to the other house of the legislature, where it is subject to the same committee and vote procedure. The bill may also be approved with amendments, in which case it is returned to the original house for re-consideration in its new form. If the bill passes both houses, it is presented to the governor for approval and enactment. In the case of a veto, the legislature may override the decision by supermajority vote in both houses.

A unique state-level panel dataset for the total numbers of bills introduced and enacted by each state legislature was collected by the author from the Book of the States for the years 1971 to 2000.[6] I collapse the legislation counts into two year session-level totals categorized into odd-numbered years ranging from 1971 to 1999.[7] As shown in Panel A of Table 3.1, approximately 3,600 bills were introduced and 860 enacted on average per legislative session over the sample time period. A large amount of variation exists across states, with a standard deviation in introductions of roughly 4,200. For this reason, legislative outcomes are taken in log form in subsequent regressions.

State budget delays have been used previously as a measure of legislative gridlock and

[6]Relevant information on legislative resolutions and bills was gathered from tables in the yearly volumes of The Book of the States for 1951-2000. From 1951 to 1954, total legislative enactments were available for each state, undifferentiated between bills and resolutions. The numbers of introductions and enactments combined for bills and resolutions were available for 1955 to 1970. Beginning in 1971, the total distinct numbers of introduced and enacted bills and resolutions were available. Therefore, the timeframe of focus is 1971 to 2000.

[7]Nine states have annual legislative sessions during the sample time period and three states have biennial legislative sessions beginning in even-numbered years during the sample time period. The remainder have biennial legislative sessions beginning in odd-numbered years.

Table 3.1: *Summary statistics*

	Mean (1)	SD (2)	Obs. (3)
Panel A. Legislative activity			
Bills introduced	3,632.5	4,268.4	678
Bills enacted	862.7	534.3	683
Late budget	0.156	0.363	1,792
Days budget late	4.96	19.99	1,792
Panel B. State direct expenditure (in millions)			
All	10,827.2	15,285.8	2,790
Public goods	5,317.6	7,469.7	2,790
Health/hospitals	872.6	1,209.0	2,790
Education	2,044.3	2,517.4	2,790
Public welfare	2,400.8	4,032.9	2,790
Panel C. Elections			
Number of candidates	1.780	0.618	105,171
Incumbent present	0.767	0.423	105,171
Any non-incumbent	0.736	0.441	105,171
Total votes	21,277.0	27,692.5	100,308
Incumbent vote share (cond. on challenge)	0.628	0.111	52,874
Incumbent victory (cond. on challenge)	0.904	0.295	52,874

Notes: Bill introduction/enactment data ranges from 1971-2000. Budget lateness data ranges from 1961-2006. Expenditure data ranges from 1951 to 2006. Elections data ranges from 1968-2010.

bargaining failure (Binder 2003, Andersen et al. 2012, Klarner et al. 2012). In addition, characteristics which one would expect to make the bargaining process and compromise more difficult, such as divided government, have been found to increase budget delays. Data on state budget lateness, as well and legislative and gubernatorial characteristics, for the 1961-2010 period was kindly provided by Carl Klarner. Table 3.1 shows that state budgets are late, i.e. adopted after the start of the fiscal year, more than 15 percent of the time, for an average lateness of 5 days. Conditional on being late, average lateness rises to 32 days.

3.3.4 State expenditure

Yearly state-level direct expenditure data are taken from the "State Government Finances" report series of the U.S. Census Bureau for the years 1951 to 2006.[8] Total yearly state expenditure is the sum of direct and intergovernmental expenditure. Intergovernmental expenditure is defined as "amounts paid to other governments as fiscal aid in the form of shared revenues and grants-in-aid, as reimbursements for performance of general government activities and for specific services for the paying government, or in lieu of taxes". Direct expenditure is officially defined by the U.S. Census Bureau as "payments to employees, suppliers, contractors, beneficiaries, and other final recipients of government payments–i.e., all expenditure other than intergovernmental expenditure". I focus on states' direct government expenditure, given its clearer interpretation and the fact that it comprises nearly 75 percent of total expenditure on average over this period.

Panel B of Table 3.1 reports summary statistics for direct expenditure in total as well as for public goods, which I follow Campate and Do (2014) in defining as the sum of health and hospitals, education, and public welfare. Total annual state direct expenditure averages roughly $10.8 billion (inflation adjusted each year to 2012 USD terms), with nearly one half of this expenditure going towards the public goods categories. As would be expected given differences in the size of states and their economies, there is also a great deal of variance in spending outcomes, so outcomes are considered in logs.

[8]Portions of the data were obtained directly from the Census Bureau as well as from Baicker et al. (2012).

3.3.5 Confidence in government

Gallup State of the Nation Surveys were conducted in 1972, 1974, and 1976, covering a nationally representative sample of voting age individuals.[9] Importantly for the purposes of this paper, these waves fall during the period in which many of the switches in open meetings occurred and each survey included identically the question: "How much trust and confidence do you have in the government of the state where you live when it comes to handling state problems?", where respondents were asked to respond on a 4-point scale ranging from "None at all" to "A great deal". I collapse the data to the state-year level, at which the mean value is 2.89, with a standard deviation of 0.83.

3.3.6 Government characteristics and election outcomes

I obtain information on candidates and vote totals for the universe of elections to state legislatures spanning the years 1968-2010 from the dataset released by Klarner et al. (2012). I restrict attention to single-member districts, which cover roughly eighty percent of all general election races.[10] As Panel C of Table 3.1 shows, an average of 1.8 candidates run in legislative races. Incumbents run for re-election in about 77 percent of races and are unopposed roughly one third of the time when they do. When facing challengers, incumbents win more than 90 percent of their re-election bids and do so by an average margin of more than 30 percentage points.

[9]Given this sampling approach, the states of Alaska, Nevada, North Dakota, and Wyoming have no residents included in any of these three waves of the survey.

[10]Multi-member districts with positions posts (i.e. specific post positions are voted for) are included in this category.

3.4 Identification

I estimate how the impacts of open meetings vary with the average log distance of the state's population to the capital city using equations of the type:

$$Y_{sct} = \mu_{sc} + \theta_c * \alpha_t + \beta_1 M_{sct} + \beta_2 [M_{sct} * D_{sc}] + X'_{sct}\lambda + \varepsilon_{sct} \tag{3.1}$$

where Y_{sct} is an outcome of interest for state s in census region c in time period t. M_{sct} is an indicator variable for the passage of a legislative open meetings law and D_{sc} is the log average distance of the population to the state capital as of 1950. State fixed effects, μ_{sc}; region-by-time-period fixed effects, $\theta_c * \alpha_t$; and X_{sct}, a vector of state-specific time varying controls are included. I cluster standard errors at the state level.

The state fixed effects control for time-invariant differences at the state level and the region-by-year fixed effects non-parametrically control for any differential trends occuring at the level of region or higher. The basic state-year level controls are log state population, log per capita income, an indicator for whether a gubernatorial election occurs in that time period, and a set of variables capturing the structure and composition of state government: sizes of the upper and lower houses, share democrat of each house, the proportion of legislators across both houses that are the same party as the governor, and indicators for divided government and the political affiliation of the governor. Specific to each set of outcomes, additional controls may be included, and are described in the corresponding section of the results.

As in Padró-i-Miquel et al. (2014), the empirical strategy is analogous to a triple differences approach. Outcomes are compared (1) within states before and after open meeting laws; (2) across states that adopt such laws and not; and (3) across states that have more or less isolated capitals. In interpreting the estimates, $\beta_1 + \beta_2 * D_{sc}$ is the effect of open meetings for a state with log average population distance to the capital D_{sc}, the value of which ranges between 0.423 and 0.848 for the United States. In the results, I will focus on the impact of open meetings at the average level of state capital isolation, $\beta_1 + \beta_2 * 0.688$, as well as how the impacts vary with isolation, i.e. whether β_2 is significantly different than

zero. Finally, since open meetings adoption is not randomly assigned, following the main results I consider an event-study specification which allows me to determine whether the timing of open meetings adoption is correlated with differential pre-trends in outcomes at the state level.

3.5 Results

3.5.1 Legislative activity

In Table 3.2, I first examine whether the introduction of open meetings influenced the behavior of legislators as reflected in the log numbers of bills introduced and enacted in session and the lateness of state budgets.[11] I find no evidence in column (1) of significant average impacts on log introductions, or differences in impact by capital isolation. Turning to log enactments in column (2), however, I observe a significant average decline of roughly 11 percent. This drop is concentrated in more isolated legislatures, where in states with lower average log distance the impact becomes positive but insignificantly different from zero.

Columns (3) and (4) examine whether open meetings influence the timeliness of state budgets. Though no there are no significant impacts on the extensive margin of budget lateness, column (4) shows that in states with low capital isolation, the average number of days that budgets are late declines with open meetings. This effect significantly attenuates as the isolation of the state capital increases, so that in more isolated locations open meetings have no effect on either measure of timeliness.

[11]For the bill-related outcomes, regressions include additional controls for whether each two year period was composed of annual (as opposed to biennial) sessions, whether for annual sessions bill consideration can carry over across one session to the next, and whether for biennial sessions the session start year is odd versus even, and whether for sessions that occur once every two years if an additional session can be called if necessary in the second year. Additionally, estimation for bill-related outcomes is conducted using Poisson regressions with the level form of variables (results are robust to the use of OLS with log form of variables). For budget lateness outcomes, regressions include additional controls for whether the budget is annual or biennial, whether the state has the line-item veto, whether a partial government shutdown is mandated if the budget is late, and the log amounts of federal aid received for education, health and hospitals, public welfare, and other categories.

Table 3.2: *Legislative activity*

	Ln bills introduced	Ln bills enacted	Budget timeliness	
			Late (0/1)	Days Late
	(1)	(2)	(3)	(4)
Open meetings * Capital isolation	0.379	-1.129**	0.244	22.23*
	(0.383)	(0.469)	(0.265)	(11.74)
Open meetings	-0.219	0.663**	-0.166	-15.61**
	(0.257)	(0.327)	(0.182)	(7.57)
Observations	633	638	1,682	1,682
Effect: mean sample isolation	0.042	-0.114**	0.002	-0.35
	(0.040)	(0.054)	(0.044)	(2.89)
Effect: min sample isolation	-0.058	0.185	-0.063	-6.20*
	(0.1000	(0.135)	(0.078)	(3.42)
Effect: max sample isolation	0.103	-0.295***	0.041	3.24
	(0.081)	(0.093)	(0.065)	(3.96)
Outcome mean [SD]	3,683.7	889.3	0.161	5.110
	[4,367.0]	[541.3]	[0.367]	[20.307]
State, Year*Region FE	X	X	X	X
Legislative controls	X	X	X	X
Population/income controls	X	X	X	X

Notes: All columns report estimates from regressions at the state level of the listed variable on an indicator for open meetings interacted with state capital isolation as of 1950. Columns (1) and (2) are based on Poisson regressions using the level form of outcomes, while columns (3) and (4) are OLS estimates. Regressions include state and session-by-census-region (or year-by-census-region in columns (3) and (4)) fixed effects and controls for log state population, log per capita income, an indicator for whether a gubernatorial election occurs in that time period, sizes of the upper and lower houses, share democrat of each house, the proportion of legislators across both houses that are the same party as the governor, and indicators for divided government and the political affiliation of the governor. Columns (1) and (2) also control for whether each two year period was composed of annual (as opposed to biennial) sessions, whether for annual sessions bill consideration can carry over across one session to the next, and whether for biennial sessions the session start year is odd versus even, and whether for sessions that occur once every two years if an additional session can be called if necessary in the second year. Columns (3) and (4) also control for whether the budget is annual or biennial, whether the state has the line-item veto, whether a partial government shutdown is mandated if the budget is late, and the log amounts of federal aid received for education, health and hospitals, public welfare, and other categories. Standard errors clustered at the state level. Additionally shown in each column are the implied effects of open meetings at the average 1950 level of isolation observed in the data across states, as well as the minimum and maximum values. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

3.5.2 State expenditure

Table 3.3 considers the effects of open meetings on log state government direct expenditure.[12] Column (1) indicates that total expenditure increases by approximately 4 percent following open meetings adoption, where insignificant differences in the effect by state isolation cannot be rejected. In column (2), I consider the impact on expenditure for public goods. Open meetings significantly increase public goods expenditure on average by more than 5 percent, and this effect is driven by larger impacts in more isolated areas, which spend less on public goods to begin with.

Columns (3) through (5) consider the impacts of open meetings separately for each of the public goods categories: health and hospitals, education, and public welfare. The pattern of coefficients is similar in each case, with the effect of open meetings being relatively more positive in states with more isolated governments, and the average impact is significantly different from zero for both education and public welfare.

3.5.3 Confidence in government

While the previous sections have focused on the outcomes over which government officials have direct influence, I next examine whether open meetings influence the opinions and behavior of citizens.[13] In Table 3.4, I ask if open meetings lead to changes in citizens' confidence in state government, as captured by the Gallup Poll question on trust and confidence in the state government's ability to handle state level problems. Column (1) presents estimates where the 4-point scale measure is used as an outcome, showing a significant average increase of 0.3 points, where higher values signify increased confidence. The coefficient on the interaction of open meetings with capital isolation is insignificant but suggestive of the effect being weaker in states with greater isolation. When considering as

[12] Regressions include additional controls for the shares of the population age 15 and below and age 65 and older, as well for the log amounts of federal aid received for education, health and hospitals, public welfare, and other categories. Also included is an indicator variable for the presence court-ordered school finance equalization (SFE) programs.

[13] Regressions additionally include a control for the share of the population 18 and older.

Table 3.3: *State direct expenditure*

	Total (1)	Public goods (2)	Health/ hospitals (3)	Education (4)	Public welfare (5)
Open meetings * Capital isolation	0.017	0.586**	0.248	0.373**	2.056
	(0.175)	(0.230)	(0.497)	(0.156)	(1.499)
Open meetings	0.031	-0.347**	-0.169	-0.203*	-1.128
	(0.121)	(0.159)	(0.355)	(0.106)	(0.994)
Observations	2,350	2,350	2,350	2,350	2,350
Effect: mean sample isolation	0.042*	0.056*	0.002	0.054*	0.287*
	(0.021)	(0.032)	(0.046)	(0.031)	(0.161)
Effect: min sample isolation	0.038	-0.099	-0.064	-0.045	-0.258
	(0.050)	(0.067)	(0.149)	(0.046)	(0.380)
Effect: max sample isolation	0.045	0.150***	0.041	0.113**	0.616*
	(0.036)	(0.051)	(0.083)	(0.043)	(0.327)
Outcome mean [SD]	15.748	14.976	13.167	14.119	13.863
	1.078]	[1.195]	[1.219]	[1.097]	[1.584]
State, Year*Region FE	X	X	X	X	X
Legislative controls	X	X	X	X	X
Population/income controls	X	X	X	X	X
Federal aid controls	X	X	X	X	X

Notes: All columns report OLS estimates from regressions at the state level of the log of state direct expenditure in the listed category on an indicator for open meetings interacted with state capital isolation as of 1950. Public goods is the sum of health/hospitals, education, and public welfare. Regressions include state and year-by-census-region fixed effects and, in addition to those listed in Table 3.2, controls for shares of the population age 15 and below and age 65 and older, as well for the log amounts of federal aid received for education, health and hospitals, public welfare, and other categories. Also included is an indicator variable for the presence court-ordered school finance equalization (SFE) programs. Standard errors clustered at the state level. Additionally shown in each column are the implied effects of open meetings at the average 1950 level of isolation observed in the data across states, as well as the minimum and maximum values. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

the outcome an indicator for choosing the top two confidence categories, a significant 20 percent average increase is observed. In addition, the coefficient on the interaction term becomes significantly negative, so that the large positive effect at low isolation becomes insignificantly different from zero at high levels of log average distance.

To shed light on where in the distribution of initial confidence the effects are concentrated, in columns (3) and (4) I examine whether there are significant shifts in the proportion of individuals expressing the highest and lowest levels of confidence following open meetings. I find no effects for the highest confidence category, but observe a great deal of heterogeneity in the effect of open meetings when the lowest category is considered as an outcome. At low values of capital isolation, open meetings leads to large and significant shifts away from the lowest category. In contrast, at high levels of isolation, the likelihood that respondents express the lowest level of confidence increases significantly with open meetings.

These results are consistent with a setting in which open meetings lead to changes in the information levels of citizens across different levels of isolation. The opposite direction of the effects in states with low and high isolation suggest that the public is not only reacting to the passage of open meetings laws per se, but to the content of what they reveal.

3.5.4 Candidate and voter behavior

Though the results thus far indicate that open meetings lead to meaningful impacts on public perception of state government, another question is whether these shifts in opinion actually lead to changes in voting behavior, or whether the decisions of incumbents and challengers to run for office are affected, in races for the houses of state legislatures.[14] Appendix Table C.1 presents the estimates of the impact of open meetings on the number of candidates, whether the incumbent runs for re-election, and if any non-incumbents run for office. For none of these variables are significant average effects, or differences in impact by log average distance, observed.

[14]Regressions additionally include a control for the share of the population 18 and older and fixed effects at the district (i.e. capturing a specific position within a house) rather than state level.

Table 3.4: *Confidence in state government*

	4-point scale	0-1 indicator		
		Confident	Highest	Lowest
	(1)	(2)	(3)	(4)
Open meetings * Capital isolation	-2.942	-1.561*	-0.034	1.347**
	(1.887)	(0.909)	(0.678)	(0.533)
Open meetings	2.323*	1.272*	0.063	-0.987**
	(1.334)	(0.645)	(0.479)	(0.381)
Observations	123	123	123	123
Effect: mean sample isolation	0.304*	0.201**	0.049	-0.063
	(0.178)	(0.086)	(0.203)	(0.050)
Effect: min sample isolation	1.077*	0.611**	0.034	-0.417**
	(0.552)	(0.268)	(0.136)	(0.160)
Effect: max sample isolation	-0.173	-0.052	0.040	0.155*
	(0.329)	(0.157)	(0.087)	(0.089)
Outcome mean [SD]	2.930	0.771	0.208	0.049
	[0.448]	[0.222]	[0.226]	[0.118]
State, Year*Region FE	X	X	X	X
Legislative controls	X	X	X	X
Population/income controls	X	X	X	X

Notes: All columns report OLS estimates from regressions at the state level of the listed variable on an indicator for open meetings interacted with state capital isolation as of 1950. Regressions include state and year-by-census-region fixed effects and, in addition to those listed in Table 3.2, controls for shares of the population age 18 and older. Outcomes are state-year level averages based on the question: "How much trust and confidence do you have in the government of the state where you live when it comes to handling state problems?", where respondents were asked to respond on a 4-point scale ranging from (1) "None at all" to (4) "A great deal". Column (1) presents estimates where the 4-point scale measure is used as an outcome. Column (2) is an indicator for choosing the top two confidence categories, while columns (3) and (4) are indicators for choosing the highest and lowest levels of confidence. Standard errors clustered at the state level. Additionally shown in each column are the implied effects of open meetings at the average 1950 level of isolation observed in the data across states, as well as the minimum and maximum values. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Table 3.5 considers, conditional on incumbent presence, the impacts of open meetings on voting. While no effects on log turnout or incumbents' vote share and likelihood of victory are observed on average, there exists significant heterogeneity in impacts by state isolation. Column (1) shows that open meetings decrease turnout significantly in states with low average log distance, but that this effect dampens as isolation increases, so that in high isolation states the effects are no longer distinguishable from zero. Turning to column (2), I observe that incumbents in states with highly isolated legislatures see a significant drop in vote share, while those low isolation areas benefit from open meetings. Finally, the absence of corresponding effects on incumbents' likelihood of winning re-election is unsurprising given how large are the margins of victory for incumbent legislators in state level elections.

Consistent with the results on confidence in government, the results further suggest that open meetings lead to shifts in voters' information and that they respond to its content. If the absence of turnout effects in more isolated areas reflected simply that the de jure increase in transparency from open meetings did not lead to any de facto increase in public information, the decrease in incumbent vote share in these areas would not be expected.

3.5.5 Identification check: event-study

Given the empirical strategy in this paper, it is important to determine whether there exist significant differential trends in outcomes between states with and without open meetings laws prior to adoption. To do so I use the following regression:

$$Y_{sct} = \mu_{sc} + \theta_c * \alpha_t + \sum_{\tau} \beta_\tau M_{\tau,sct} + \sum_{\tau} \varphi_\tau [M_{\tau,sct} * D_{sc}] + X'_{sct}\lambda + \varepsilon_{sct} \tag{3.2}$$

where $M_{\tau,sct}$ is a vector of indicator variables for whether in state s period t falls τ periods relative to open meetings adoption. Observations in the period prior to open meetings adoption ($\tau = -1$) serve as the reference group. All observations that occur three or more periods prior to adoption are grouped together, as are all observations that are five or more

Table 3.5: *Voting outcomes*

	Ln voter turnout (1)	Incumbent vote share (2)	Incumbent victory (3)
Open meetings * Capital isolation	0.282*	-0.121**	-0.040
	(0.163)	(0.050)	(0.095)
Open meetings	-0.226**	0.079**	0.034
	(0.111)	(0.032)	(0.064)
Observations	65,185	68,692	68,692
Effect: mean sample isolation	-0.032	-0.005	0.005
	(0.025)	(0.005)	(0.006)
Effect: min sample isolation	-0.106**	0.027**	0.016
	(0.047)	(0.012)	(0.024)
Effect: max sample isolation	0.013	-0.024**	-0.002
	(0.038)	(0.012)	(0.019)
Outcome mean [SD]	9.447	0.759	0.939
	[0.960]	[0.198]	[0.239]
State, Region*Year FE	X	X	X
Legislative controls	X	X	X
Population/income controls	X	X	X

Notes: All columns report OLS estimates from regressions at the district level of the listed variable on an indicator for open meetings interacted with state capital isolation as of 1950. Regressions include district and period-by-census-region fixed effects and, in addition to those listed in Table 3.2, controls for shares of the population age 18 and older and number of candidates. Sample restricted to observations with an incumbent running for re-election. Standard errors clustered at the state level. Additionally shown in each column are the implied effects of open meetings at the average 1950 level of isolation observed in the data across states, as well as the minimum and maximum values. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

periods after adoption.[15] For non-adopting states, $M_{\tau,sct}$ is set to zero for all τ, to assist in the identification of the fixed effects and control coefficients (Greenstone and Hanna, 2014). Other variables remain unchanged from the baseline equation.

Inspection for negative values of τ of the pattern of estimates of the impact of open meetings at a given level D of state capital isolation in period τ, $\beta_\tau + \varphi_\tau * D$, allows me to establish whether there exist significant pre-trends in outcomes. This analysis, however, excludes the government confidence variables, for which only three time periods are available.

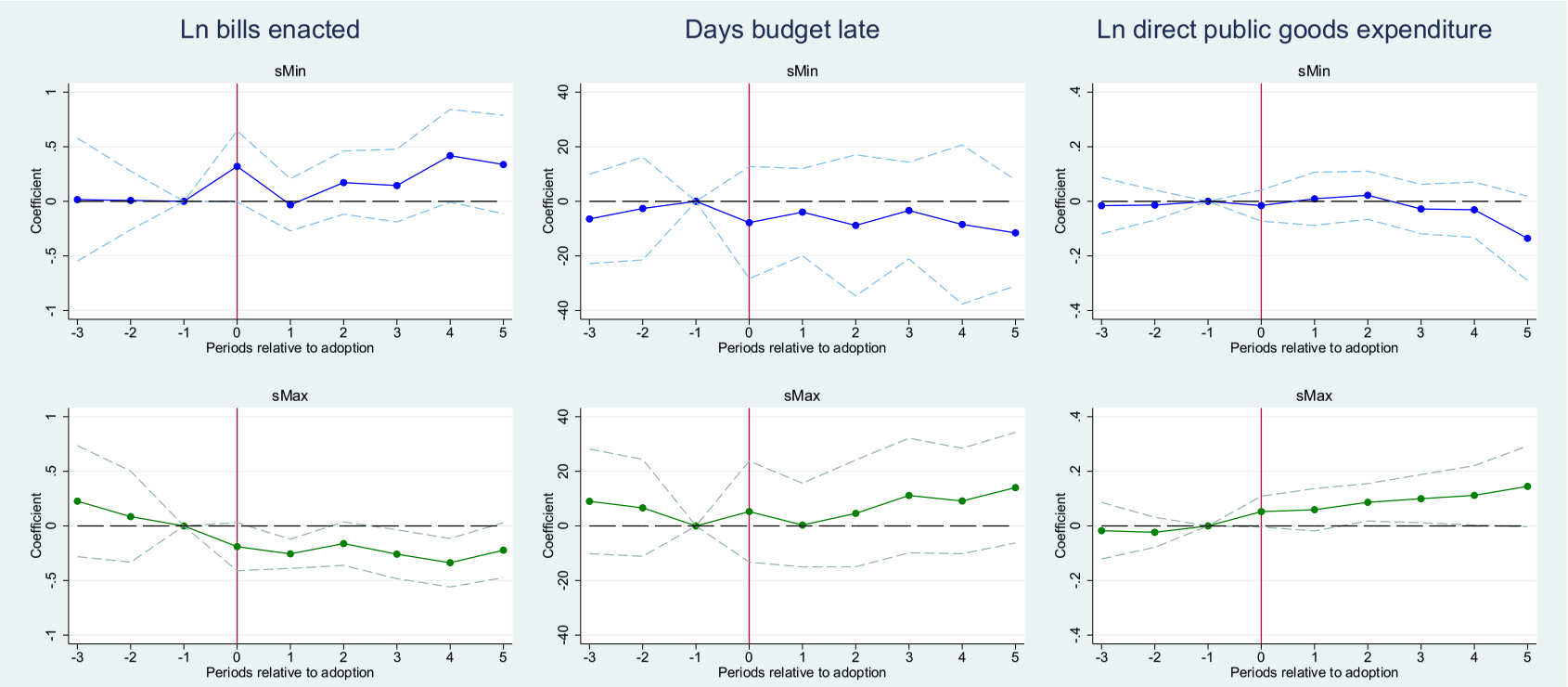
Returning the event study results, Figures 3.3 and 3.4 plot, both for the minimum and maximum values of D among the U.S. states, the estimated impact of open meetings over time for each outcome where significant effect heterogeneity by log average distance was observed previously. The 95 percent confidence intervals are also included. Reassuringly, for none of the five outcomes, at either high or low values of D, are significant impacts of open meetings observed in periods prior to adoption. In addition, examining the pattern of post-period coefficients allows for an assessment of whether the impacts of open meetings are short-lived or persistent. The plots demonstrate that across the legislative, expenditure, and election outcomes, the effects of open meetings do not attenuate significantly over time.

3.6 Conclusion

The findings of this paper suggest that open meetings in the U.S. state legislative setting are on average beneficial. Expenditure on public goods increases, especially in areas where spending of this type is the lowest to start. Additionally, citizens express greater confidence, at least in the short run, in the ability of state governments.

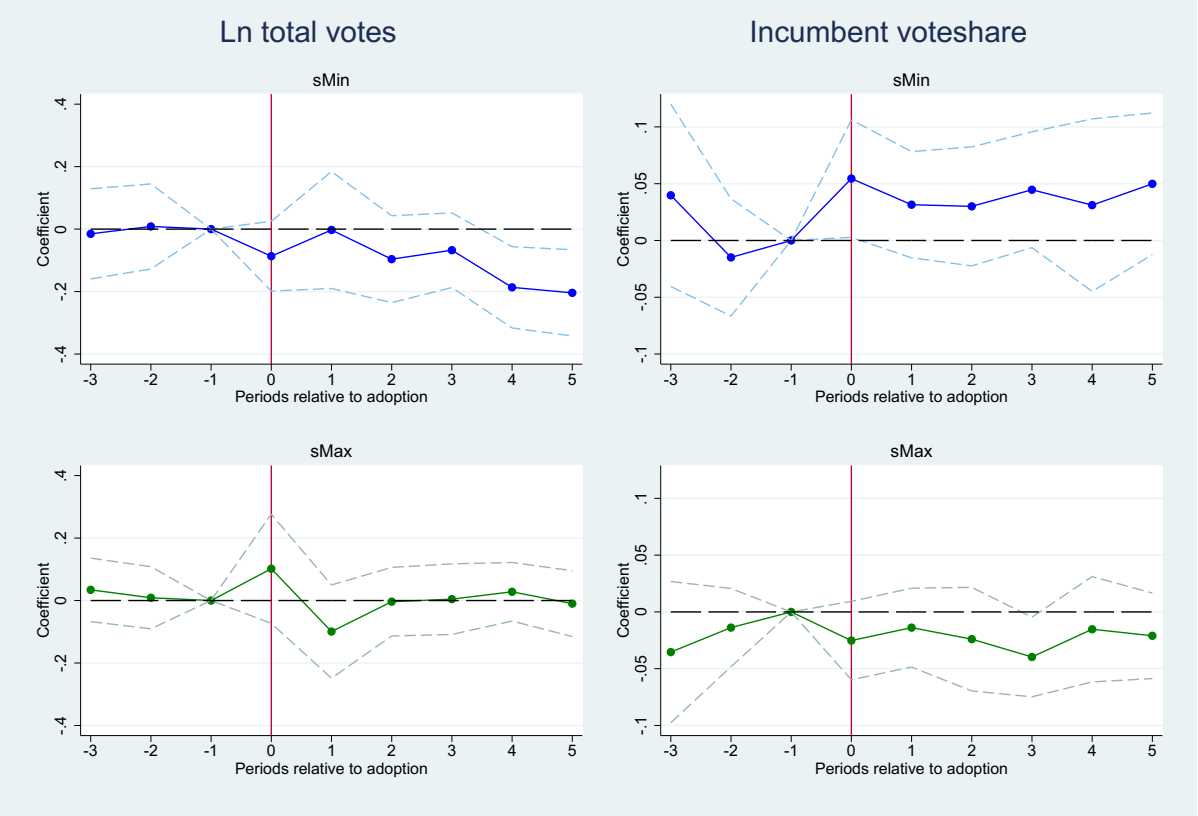
The results also indicate that even in environments with low initial levels of accountability, open meetings have significant impacts. That is, weak transmission mechanisms to the public or low uptake by citizens do not appear to be binding constraints which prevent

[15]Given the timing of the bulk of the adoptions of open meetings, a larger number of post- than pre-periods are generally available.



Notes: Figures provide a graphical analysis of the effect of open meetings on the variable listed at the top of each column, based estimates from equation (3.2). The top row presents the implied effects at the minimum level of state capital isolation observed across states in 1950. The bottom row presents the implied effects at the maximum observed level. See the text for further details. Dotted lines represent 95% confidence intervals.

Figure 3.3: *Event study of legislative and expenditure outcomes*



Notes: See notes for Figure 3.3.

Figure 3.4: *Event study of election outcomes*

increased accessibility of information about legislator behavior from producing downstream effects.

The observed drop in voter turnout in states with less isolated capitals after open meetings contrasts with the results of a number of papers examining the voting effects of increasing the provision of information about elected officials. Stromberg (2004) finds a 1.2 percent increase in votes per capita in U.S. gubernatorial elections for a 10 percent increase in county radio penetration between 1920 and 1930. Snyder and Stromberg (2010) estimate an increase in turnout for national House elections associated with greater congruence between newspaper and political markets. In the Indian election setting, Banerjee et al. (2011) find that the provision of report cards with information on incumbent performance yields a 3.5 percent rise in voter turnout. It should be noted however that the model of voting in the latter paper also suggests that the impact on turnout of increasing voter information may critically depend on the distribution of priors. For example, if open meetings lead the perceptions of citizens previously opposed to an incumbent to improve, they may shift to being indifferent between candidates, no longer view voting as having a positive expected value, and choose not to vote.

Finally, a limitation of this paper is that it does not provide evidence on shifts in the content of media coverage, which is implicitly assumed to be the link between open meetings and the observed changes in citizen confidence and voting behavior. Unfortunately, the time period during which the majority of the changes in open meetings took place (the 1970s) is one for which there is limited searchable newspaper data systematically available across a large number of states. If data of this type were to become available in the future, testing for changes in the media coverage of state politics would be a useful extension.

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Appendix A

Appendix to Chapter 1

A.1 Survey sampling

A.1.1 Registered voters survey

Polling stations in urban areas, where locating specific individuals based on the information available in the electoral roll would not have been feasible, were excluded from the sample (8.3 percent). Additionally excluded were polling stations with only three election officers (0.7 percent), as were polling stations that were split across a main polling station and an extension station (9.8 percent). The list of registered voters was at the (main+extension) level, so it was not possible to determine to which of the main station or extension individuals were assigned. The only difference between having a main and extension station versus two polling stations in the same location is whether the threshold for maximum registered voters at a single station was reached after the formal yearly deadline to split polling stations. Administration is otherwise identical.

In some locations, fewer than three Muslims or two Yadavs were identified in the list. If too few Muslims were available, Yadavs were randomly drawn to fill the positions when possible, and vice versa. If fewer than five Muslims and Yadavs in total were identified, individuals that were neither Muslim nor Yadav were randomly drawn to fill the position.

Seasonal migration is common in the survey area and the electoral rolls contain errors

(e.g. listed individuals may be duplicates or have moved and registered at another polling station without being deleted from the list at the previous station). Therefore, randomly drawn backup respondents were also identified for each primary respondent. In the final sample, 36.6 percent of respondents were from the primary sample, 22.6 percent were the first backup, 14.6 percent were the second backup, 11.2 percent were the third backup, and 15 percent were fourth backup or higher. These rates of replacement are similar to those of other surveys in the region which identified respondents based on the electoral roll (Banerjee 2014). The rate of primary versus backup respondents does not differ significantly by whether the polling station is mixed versus homogeneous team. The consent rate among located respondents was very high, with more than 98.5 percent of individuals agreeing to participate. If an individual indicated that they did not go to the polling station to attempt to vote on election day, the next backup individual was then substituted.

A.1.2 Election officers survey

A total of 6,251 officers served at polling stations during the 2014 election in the district in which the survey was conducted. Out of these officers, 6,045 had phone numbers listed in the administrative data which were not obviously incorrect (i.e. having the wrong number of digits or all zero numerals). Of these 6,045 individuals, 614 officers were inferred as Muslim or Yadav. Each of these individuals was attempted to be reached by phone. One non-Muslim/Yadav officer was randomly selected for calling from each of the mixed composition teams of which the previous 614 Muslim/Yadav officers were a member. If the officer could not be reached or did not consent, another non-Muslim, non-Yadav officer was selected as a replacement, if possible. An additional 600 homogeneous polling teams were randomly chosen and an officer from within the team was randomly selected. Again, if the officer could not be reached or did not consent, another officer was selected as a replacement, if possible. A total of 2,350 officers were called in total. In 30 percent of instances the individual was not reachable (in the vast majority of cases due to the listed phone number not being functional). Willingness to participate was very high among the officers who were

reachable, with only 2 percent (33) of officers not consenting to be surveyed in the future. Calling yielded 380 mixed team polling stations with at least one Muslim/Yadav officer and non-Muslim/Yadav officer each confirmed as consenting and 436 homogeneous polling stations with at least one officer confirmed as consenting, from which 305 mixed team and homogeneous pollling stations each were randomly selected as described in the main text.

A.2 Vignette experiment names and list experiments prompt

A.2.1 Vignette experiment names

Muslim: Najam Uddin, Mustak Ansari, Mohammed Alam

Yadav: Ajay Yadav, Kailesh Yadav, Surendra Yadav

Brahmin: Arjun Tripathi, Rohit Mishra, Alok Chaturvedi

A.2.2 List experiments prompt

"I'm going to read you a list of various statements, and I would like for you to tell me how many of them occurred during the previous 2014 Lok Sabha election. Please, count to yourself. Do not tell me which ones, only HOW MANY IN TOTAL. For example, it might be that none of them occurred, all of them occurred, or any number in between."

A.3 Counterfactual calculation details

The total estimated effect on the RJD-BJP vote share margin of shifting to a mixed composition polling team is the sum of the within-station effect and the cross-station spillover effect multiplied by the number of neighbor polling stations, adjusting for the sub-constituency level of voter identity card coverage, ID_c. Using available sub-constituency-level administrative data for the entire state of Bihar, I calculate the average number of neighbors for a polling station in each sub-constituency, N_c. Taking the coefficients from a modified version of equation (1.2) allowing for heterogeneity by identity card coverage, estimated on the

sample districts for which I possess officer assignment information:

$$Y_{pc} = \mu_c + \theta_o + \beta Mixed_{pc} + \gamma T_{pc} + \phi N_{pc} + \beta_2 \left[Mixed_{pc} * ID_c\right] + \gamma_2 \left[T_{pc} * ID_c\right] + \phi_2 \left[N_{pc} * ID_c\right] + \mathbf{X'_{pc}}\lambda + \epsilon_{pc}$$

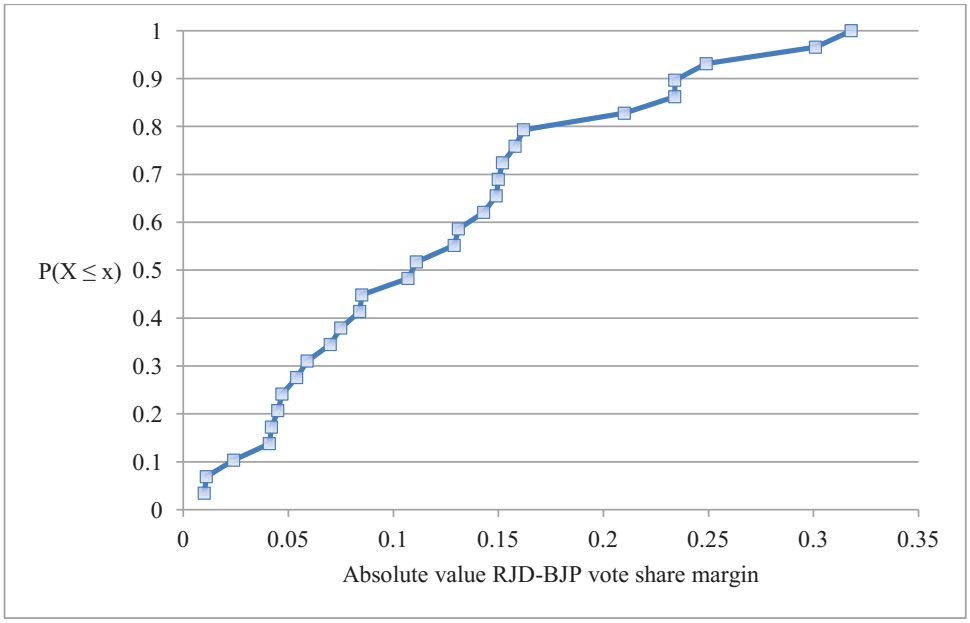
the impact of a change of magnitude, X, in the proportion of mixed polling stations in a sub-constituency can be estimated as $X * [(\beta + \gamma * N_c) + (\beta_2 + \gamma_2 * N_c) * ID_c]$. While I do not observe the actual baseline proportion of mixed teams outside of my sample area, the value of X needed to change the outcome of the race between the RJD and BJP coalitions can be calculated using the formula above together with the constituency level margins of victory. When calculating impacts at the parliamentary constituency level, I take a weighted average (based on number of polling stations) across the sub-constituencies within that parliamentary constituency. The impacts of alternative team composition scenarios can then be assessed based on the range within which one assumes the baseline proportion of mixed team polling stations in each constituency falls. I assume that the baseline proportion in all sub-constituencies is the same as that in the observable sample, 0.324.

A.4 Supplemental figures and tables

Figure A.1: *Example of polling officer team during election day proceedings*

Figure A.2: *Example of government-issued voter identity card*

Figure A.3: *Example of neighboring polling stations in close proximity*



Notes: Figure plots the empirical cumulative distribution function of the absolute value of the parliamentary-constituency-level vote share margin between the RJD and BJP coalitions, for the 29 of 40 races where these two coalitions fielded the top two candidates.

Figure A.4: *Cumulative distribution function of coalition vote share margins*

Table A.1: *Randomization check - spatial characteristics*

	Homog. team (1)	Mixed team (2)	Difference (3)	p-value (4)	Obs. (5)
Number mixed team neighbor stations	0.385	0.386	-0.012	0.493	5,561
	[0.746]	[0.719]	(0.018)		
Total neighbor stations	1.200	1.191	-0.027	0.420	5,561
	[1.614]	[1.647]	(0.034)		
Number mixed team stations within 0.25km	0.420	0.452	0.026	0.392	5,097
	[1.078]	[1.159]	(0.030)		
Number mixed team stations within 0.25-0.75km	2.536	2.622	0.066	0.430	5,097
	[4.263]	[4.470]	(0.084)		
Total stations within 0.25km	1.357	1.336	-0.025	0.735	5,097
	[2.930]	[2.904]	(0.073)		
Total stations within 0.25-0.75km	7.893	7.958	0.069	0.768	5,097
	[12.830]	[12.904]	(0.232)		
Number mixed team stations within village	1.210	1.309	0.043	0.607	3,231
	[2.178]	[2.287]	(0.083)		
Number mixed team stations in neighboring villages	4.688	4.829	-0.040	0.768	3,216
	[3.908]	[4.015]	(0.136)		
Total stations within village	3.686	3.812	0.088	0.676	3,231
	[5.551]	[5.868]	(0.212)		
Total stations in neighboring villages	14.259	14.479	0.065	0.861	3,216
	[10.694]	[10.544]	(0.369)		

Notes: Columns (1) and (2) report variable means with standard deviations in brackets for homogeneous and mixed officer teams. Column (3) reports the coefficient from an OLS regression where the listed outcome is regressed on an indicator for polling station mixed team composition and column (4) reports the associated p-value. Also included are sub-constituency and number of officer fixed effects. Neighbor stations are polling stations within the same building/compound of a given polling station. Stations with 0.25 and within 0.25-0.75km are non-neighbor stations within 0.25km and 0.25-0.75km of a given polling station, respectively. Numbers of stations within a village and in neighboring villages are the numbers of non-neighbor polling stations within the same village as a given polling station and in villages adjacent to a given polling station's village, respectively. Sample is restricted to those polling stations matched to the dataset of polling station GPS locations. Village-related outcomes further exclude stations in villages which are in the top 1 percent of the distribution in terms of number of polling stations contained within, or their neighboring villages.*Significant at 10% level **5% level ***1% level.

Table A.2: *Cross-position balance*

	Presiding officer (1)	Polling officer 1 (2)	Polling officer 2 (3)	Polling officer 3 (4)	Polling officer 4 (5)
Muslim/Yadav presiding officer		-0.006	0.006	-0.004	-0.016
		(0.014)	(0.014)	(0.014)	(0.030)
Muslim/Yadav polling officer 1	-0.005		-0.004	-0.019	-0.015
	(0.013)		(0.013)	(0.013)	(0.027)
Muslim/Yadav polling officer 2	0.006	-0.004		0.014	-0.009
	(0.014)	(0.014)		(0.015)	(0.027)
Muslim/Yadav polling officer 3	-0.003	-0.018	0.012		-0.020
	(0.013)	(0.012)	(0.014)		(0.029)
Muslim/Yadav polling officer 4	-0.014	-0.013	-0.009	-0.017	
	(0.026)	(0.030)	(0.031)	(0.025)	
Observations	5,561	5,561	5,561	5,523	1,178

Notes: Each column reports coefficients from an OLS regression where the outcome is Muslim/Yadav assignment to the specified position, and is regressed on dummies for Muslim/Yadav assignment to the other polling officer team positions specified in table. Additionally included are sub-constituency and number of officer fixed effects. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Table A.3: *Position- and number-specific impacts on voting outcomes*

	Ln votes RJD	Ln votes BJP	Vote share margin RJD-BJP	Ln total votes
	(1)	(2)	(3)	(4)
Panel A. Position				
Muslim/Yadav presiding officer	-0.006	-0.017	0.007	-0.012
	(0.052)	(0.043)	(0.020)	(0.018)
Muslim/Yadav polling officer 1	0.088*	-0.013	0.031*	0.017
	(0.050)	(0.037)	(0.019)	(0.011)
Muslim/Yadav polling officer 2	0.050	-0.064	0.021	0.002
	(0.052)	(0.044)	(0.020)	(0.014)
Muslim/Yadav polling officer 3	0.054	-0.086**	0.037*	0.000
	(0.050)	(0.040)	(0.019)	(0.019)
Muslim/Yadav polling officer 4	0.111	-0.001	0.041	0.001
	(0.189)	(0.170)	(0.087)	(0.033)
F-test p-value: equality of coeffs.	0.731	0.612	0.824	0.614
Observations	5,276	5,290	5,293	5,293
Panel B. Number				
Any Muslim/Yadav officer	0.055*	-0.046**	0.027**	0.002
	(0.028)	(0.022)	(0.011)	(0.008)
Multiple Muslim/Yadav officers	-0.061	0.040	-0.024	-0.010
	(0.061)	(0.053)	(0.024)	(0.018)
Observations	5,535	5,535	5,549	5,549

Notes: All columns in Panel A report OLS estimates from regressions at the polling station level of the listed variable on indicators for Muslim/Yadav presence in each polling party position, conditional on there being 1 or fewer total MY officers at the polling station. All columns in Panel B report OLS estimates from regressions at the polling station level of the listed variable on indicators for the degree of Muslim/Yadav presence. Additionally included in all regressions are sub-constituency and number of officer fixed effects and controls for the log number of registered voters at the polling station and the Muslim/Yadav share of registered voters *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Table A.4: *Heterogeneity in impacts of team composition by electorate composition*

	Ln votes RJD (1)	Ln votes BJP (2)	Vote share margin RJD-BJP (3)	Ln total votes (4)
Mixed team	0.038	-0.023	0.018	0.003
	(0.037)	(0.027)	(0.014)	(0.010)
Mixed team * Muslim/Yadav registered voter %	0.057	-0.131	0.039	-0.014
	(0.158)	(0.166)	(0.053)	(0.039)
Muslim/Yadav registered voter %	0.031***	-0.029***	0.015***	-0.000*
	(0.001)	(0.001)	(0.000)	(0.000)
Observations	5,535	5,549	5,552	5,552

Notes: Each column reports OLS estimates from regressions at the polling station level of the listed outcome on indicators for mixed team composition, interacted with a continuous measure of the polling station level proportion of registered voters that are Muslim or Yadav. Additionally included are sub-constituency and number of officer fixed effects and a control for log total registered voters. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Table A.5: *Cross-station spillovers - extended range*

	Ln votes RJD (1)	Ln votes BJP (2)	Vote share margin RJD-BJP (3)	Ln total votes (4)
Panel A. Buffer radius				
Mixed team composition	0.062**	-0.060***	0.033**	-0.001
	(0.028)	(0.022)	(0.011)	(0.008)
Number mixed team neighbor stations	0.024	-0.049**	0.026***	-0.001
	(0.026)	(0.020)	(0.010)	(0.008)
Number mixed team stations within 0.25km	0.012	0.014	-0.006	-0.004
	(0.026)	(0.014)	(0.009)	(0.007)
Number mixed team stations within 0.25-0.75km	-0.006	0.005	-0.003	0.000
	(0.009)	(0.008)	(0.004)	(0.002)
Total neighbor stations	-0.028**	0.028***	-0.021***	-0.011***
	(0.013)	(0.010)	(0.005)	(0.003)
Total stations within 0.25km	-0.028**	0.007	-0.007**	-0.006**
	(0.011)	(0.006)	(0.003)	(0.003)
Total stations within 0.25-0.75km	0.001	0.007***	-0.003*	0.001
	(0.004)	(0.003)	(0.001)	(0.001)
Observations	5,074	5,087	5,090	5,090
Number locations	3,307	3,307	3,307	3,307
Panel B. Village boundaries				
Mixed team composition	0.083**	-0.070**	0.037**	0.010
	(0.038)	(0.031)	(0.015)	(0.010)
Number mixed team neighbor stations	0.069	-0.108***	0.048**	0.011
	(0.054)	(0.041)	(0.019)	(0.011)
Number mixed team stations within village	-0.017	-0.044	0.010	-0.003
	(0.039)	(0.029)	(0.016)	(0.008)
Number mixed team stations in neighboring villages	0.008	-0.020*	0.009	0.004
	(0.013)	(0.011)	(0.006)	(0.003)
Total neighbor stations	-0.033	0.102***	-0.044***	-0.001
	(0.033)	(0.026)	(0.012)	(0.010)
Total stations within village	0.016	0.016	-0.004	-0.000
	(0.019)	(0.013)	(0.007)	(0.003)
Total stations in neighboring villages	0.000	0.011**	-0.004*	0.000
	(0.005)	(0.004)	(0.002)	(0.001)
Observations	3,196	3,210	3,212	3,212
Number villages	1,247	1,247	1,247	1,247

Notes: Each column within a panel reports OLS estimates from a regression at the polling station level of the listed variable on an indicator for mixed team composition. Each regression includes sub-constituency and number of officer fixed effects and controls for log total registered voters and share Muslim/Yadav registered voters. Neighbor stations are those within the same building/compound of a given polling station. Stations within 0.25 and 0.25-0.75km are non-neighbor stations within the stated distance of a given polling station. Numbers of stations within a village and in neighboring villages are the numbers of non-neighbor polling stations within the same village as a given station and in villages adjacent to a given station's village. Panel A is restricted to stations matched to the dataset of station GPS locations. Panel B further excludes stations in the top 1 percent of villages in terms of number of stations contained within, or their neighboring villages. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Table A.6: *Type-specific impacts of officer identity on voting outcomes*

	Ln votes RJD (1)	Ln votes BJP (2)	Vote share margin RJD-BJP (3)	Ln total votes (4)
Any Muslim officer	0.051*	-0.034	0.023**	0.011
	(0.030)	(0.024)	(0.012)	(0.008)
Any Yadav officer	0.070	-0.100**	0.044**	-0.033
	(0.057)	(0.044)	(0.022)	(0.025)
Muslim/Yadav registered voter %	0.031***	-0.030***	0.015***	-0.000**
	(0.001)	(0.001)	(0.000)	(0.000)
Ln total electors	1.007***	1.198***	-0.069***	0.934***
	(0.062)	(0.050)	(0.024)	(0.019)
Control Mean [SD]	4.451	5.143	-0.181	6.180
	[1.198]	[0.969]	[0.452]	[0.402]
Observations	5,276	5,290	5,293	5,293

Notes: All columns report OLS estimates from regressions at the polling station level of the listed variable on indicators for Muslim and Yadav presence, conditional on there being 1 or fewer total Muslim/Yadav officers at the polling station. Additionally included are sub-constituency and number of officer fixed effects and controls for Muslim/Yadav share of registered voters and log total registered voters. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Table A.7: *Cross-election impacts of randomized team composition on voting outcomes*

	Ln votes RJD 2015 (1)	Ln votes BJP 2015 (2)	Vote share margin RJD-BJP 2015 (3)	Ln total votes 2015 (4)
Mixed team	-0.016	-0.012	0.001	0.004
	(0.043)	(0.043)	(0.020)	(0.012)
Number mixed team neighbor stations	0.005	-0.067	0.035	0.025
	(0.058)	(0.058)	(0.025)	(0.015)
Total number neighbor stations	-0.107***	-0.011	-0.045***	-0.054***
	(0.036)	(0.043)	(0.015)	(0.009)
Muslim/Yadav elector %	0.020***	-0.026***	0.013***	0.000
	(0.001)	(0.002)	(0.000)	(0.000)
Ln total electors	0.969***	0.858***	0.015	0.819***
	(0.146)	(0.147)	(0.062)	(0.047)
Observations	1,335	1,332	1,335	1,335
Number locations	1,008	1,007	1,008	1,008
Homogeneous team mean [SD]	5.096	5.106	-0.010	6.243
	[0.908]	[0.973]	[0.433]	[0.321]

Notes: All columns report OLS estimates from regressions at the polling station level of the listed variable from the 2015 elections on an indicator for mixed team composition and variables for the numbers of total and mixed composition team neighboring polling stations in the 2014 elections. Additionally included are AC and number of officer fixed effects and controls for Muslim/Yadav share of registered voters and log total registered voters from 2014. Neighbor stations are polling stations within the same location (building/compound) as a given polling station. Standard errors clustered at the location level. The sample is restricted to the district where only minor changes were made to the polling station locations between the 2014 and 2015 elections. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Appendix B

Appendix to Chapter 2

Table B.1: *Differential initial trends check*

	State level
	Difference in log contract value
	(1)
Panel A. India	
E-procurement	0.0130
	(0.0378)
Mean Dep. Var.	0.202
	(0.697)
Observations	24
Panel B. Indonesia - Works Projects	
E-procurement (IV)	-0.084
	(0.115)
Mean Dep. Var.	0.086
	(0.490)
Observations	24
Panel C. Indonesia - Consultancy Projects	
E-procurement (IV)	0.017
	(0.235)
Mean Dep. Var.	-0.576
	(0.790)
Observations	21

Notes: India - Column (1) reports an OLS estimate at the state level, where the dependent variable is the difference in log average contract value between 2000 and 2001. The independent variable is the year of official adoption of electronic procurement at the state level, conditional on adoption occurring after 2001. Indonesia - Column (1) reports OLS estimates at the state level, where the dependent variable is the difference in log average contract value between 2005 and 2004. The independent variable is the year of official adoption of electronic procurement at the state level, conditional on adoption occurring after 2005. Robust standard errors are in parentheses below estimates. Non-e-procurement project means for each dependent variable are also reported, with standard deviations in parentheses. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.

Appendix C

Appendix to Chapter 3

Table C.1: *Candidate entry*

	Number candidates (1)	Non-incumbent present (2)	Incumbent present (3)
Open meetings * Capital isolation	-0.106	0.126	0.015
	(0.241)	(0.159)	(0.119)
Open meetings	0.041	-0.100	0.007
	(0.165)	(0.105)	(0.075)
Observations	89,347	89,347	89,347
Effect: mean sample isolation	-0.032	-0.013	0.018
	(0.030)	(0.020)	(0.014)
Effect: min sample isolation	-0.004	-0.047	0.014
	(0.067)	(0.041)	(0.027)
Effect: max sample isolation	-0.049	0.007	0.020
	(0.051)	(0.036)	(0.029)
Outcome mean [SD]	1.775	0.730	0.769
	[0.621]	[0.444]	[0.421]
State, Region*Year FE	X	X	X
Legislative controls	X	X	X
Population/income controls	X	X	X

Notes: All columns report OLS estimates from regressions at the state level of the listed variable on an indicator for open meetings interacted with state capital isolation as of 1950. Regressions include district and period-by-census-region fixed effects and, in addition to those listed in Table 3.2, controls for shares of the population age 18 and older. Standard errors clustered at the state level. Additionally shown in each column are the implied effects of open meetings at the average 1950 level of isolation observed in the data across states, as well as the minimum and maximum values. *Significant at 10 percent. **Significant at 5 percent. ***Significant at 1 percent.